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Psychological Bulletin

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Psychological Bulletin

RECENT RESEARCH ON PAIN PERCEPTION[1]

WARD EDWARDS
Harvard University

This paper is an outline of four related problems: the definition of pain, the psychophysics of pain, the peripheral nervous mechanisms involved in pain perception, and the role of the central nervous system in certain abnormalities of pain perception.[2]

I. Definition

The literature on pain has few discussions of the problem of its definition. The prevailing view is outlined by Lewis in the introduction to his book, *Pain* (48):

> Pain, like similar subjective things, is known to us by experience and described by illustration. . . .
>
> We have no knowledge of pain beyond that derived from human experi-

[1] It is a pleasure to thank Dr. J. G. Beebe-Center, who interested me in this problem and gave me valuable advice throughout the preparation of this review, and Dr. E. G. Boring, who gave the manuscript a careful and critical reading. Their good advice led to the correction of many errors; those which remain must be attributed to my shortcomings.

[2] Several reviews of work on pain are available in the psychological literature, but none of them is very recent and none is exhaustive. Morgan's chapter on the subject in his *Physiological Psychology* (55) is the most recent review (1943) and deals mostly with the problem of pain receptors. Dallenbach (14), writing in 1939, covered thoroughly the history of the distinction between pain and unpleasantness, as well as dealing more briefly with other topics. Stone and Jenkins (74), in 1940, covered experimental work on pain and temperature during the 1930's. In the medical and physiological literature, several more recent reviews are available. An excellent review of the skin senses in general was published by Bishop (7) in 1946. In 1947, Wolff and Hardy (92) published another very good review. Wolff and Wolf (98) covered much the same ground in a short book in 1948. Wolff's earlier Harvey Lectures (89) are by now out of date, since they were published in 1944. Lewis's (48) book is a review of his own experiments for the most part (1942). Riddoch (63) reviewed the literature on phantom limbs in 1941, and Walshe (82) reviewed epicritic and protopathic sensibility in 1942. This paper was written to bring the reviews up to date, to make available in the psychological literature the results of medical and physiological research on pain insofar as they bear on psychological issues, and to propose some criticisms and some integrations which are not to be found in the medical literature.

ence; yet we may judge of its presence in animals by bodily reactions that human experience has brought us to recognize as its frequent accompaniments or by the use of stimuli that similar experience tells us should be painful. . . . Strictly speaking, there are no reliable and usable indices of pain in animals; there are only phenomena recognized as frequent associations of pain. . . . Our knowledge of pain, of its sources, of the nerve paths conveying it, has been built up, and will continue to be built up, chiefly by observations upon man. . . . There is manifest wisdom in seeking knowledge from the direct, rather than from the indirect source, and here the direct source is prolific; there is, too, the deep satisfaction of knowing that any evidence won from man is indubitably applicable to the human problem (48, p. 1).

Many of the writers whose work is reviewed in this paper have stated views like Lewis's (e.g. 50, 92), and none of them has expressly disagreed with this essentially introspective definition of pain. This unanimity probably can be accounted for by the fact that the literature on pain is a medical literature. It seems likely that many behavioristically oriented American psychologists would prefer an objective, operational definition.

In the search for an operational definition of pain, the first step must be an examination of the criteria used by researchers in their work. By far the most commonly used criterion of pain is the verbal report (29, 48, 53). Occasionally, voluntary withdrawal of the stimulated area has been used as the criterion of pain in human beings (12, 67). In animals, the most common criteria are cries, squeaks, or squeals; reflex responses of the area stimulated; struggling; and flight (24). Not always, however, do researchers rely on behavioral evidence that their stimuli are painful —or at any rate excite afferent pain fibers. It is possible to apply stimuli to nerves known on other evidence to carry pain fibers and to call these stimuli painful, whether or not they produce reports of pain. Or it is possible to record pain impulses directly from nerves carrying pain fibers (66) and to use these as the criterion of pain, or to apply stimulation to areas considered sensitive only to pain (78). Or a certain stimulus could arbitrarily be defined as painful on the basis of some other property it has—for instance, that it produces tissue damage, or that it is negatively reinforcing.

All the above may be *sufficient* conditions for use of the word *pain.* But a definition of a word consisting of a list of different situations sufficient for its use is not ideal. It would be much more desirable to be able to state a *necessary* condition for use of the word. If *pain* refers to something specific (as common sense urges that it does) then that necessary condition must be presence of the thing to which the word refers. What is that thing? Common sense says, the experience of pain.

If we continue to use the common-sense notion of pain, it is clear

that no one of the above experimental criteria is adequate as a necessary criterion for use of the word. All of them can be shown to be absent in cases in which pain is present. Even excitation of peripheral pain fibers is absent in the case of "central pain" (**25, 53**). The alternatives, then, are to decide that pain is not inevitably connected with any of its behavioral criteria or else to abandon the common-sense notion of pain. The medical literature, and therefore this review, prefers the former choice.

The basic fact here is that common sense, and therefore the pain literature, is dualistic in its epistemology. Pain experience is different from pain-produced behavior, and it is to the former that *pain* refers. This is an instance of a general problem of epistemology which has also arisen in interpreting research on other senses; it becomes particularly acute in pain research only because the stimulus for pain is so much less clearly understood than is that for other kinds of sensory experience. In other sense modalities, a particular kind of stimulation can be considered the criterion for use of a word referring to experience (e.g., light stimulation applied to an organism causes us to speak of color vision), and the few exceptions in which the experience occurs without the stimulus can be ignored, especially since they do not usually occur under the conditions of most experiments. In pain, no such simple dependence on an easily identifiable stimulus exists. For this reason, no operational definition of pain can be proved adequate until some generalizing operation can be found which will indicate identity among different cases of pain perception, and so justify in operational terms the use of the same word for these different cases. Whether such operations exist for other sense modalities is a debated question.

If pain is subjective, how can it be experimented on? Stimuli, responses, and experimental operations are used as indicators whose meaning may vary depending on the total situation in which they are observed. A wide variety of observations are made and interpreted by each experimenter in terms of his own common sense, generated from his past experience of pain, its stimuli, and its consequences. This procedure is subject to a variety of interpretations consistent with a variety of epistemological biases. But the basic requirements for any research are reproducible results and meaningful conclusions, and so long as these methods meet these requirements they are, in the opinion of this reviewer, entirely adequate.

The day will presumably come when functional relations involving indicators can be integrated by means of a neural model without any reference to the subjective concept of pain. Will this lead to drop-

ping the subjective concept of pain from psychology? Perhaps. The history of vision, however, suggests that the answer is no. Despite advanced knowledge of physiological mechanisms in that field the concept of visual sensation is still the principal basis of organization for visual psychophysics.

II. Distinctions

A number of terms, most of them referring to dichotomies, have been used in the pain literature. A summary of these terms before discussion of the literature will provide a convenient entering place and a necessary vocabulary.

Pain-Unpleasantness. The distinction between pain and unpleasantness is fundamental to present-day thinking on the subject of pain. The word pain is now used to refer to a perception, like a tone or a color, rather than an affective state or a performance in a choice situation. It is in this sense that the word is used throughout this review. The history of the emergence of this viewpoint was reviewed by Dallenbach (**14**) in 1939.

Pain Perception–Pain Reaction. Recent workers in the field, particularly Hardy, Wolff, and Goodell, have emphasized the distinction between pain as perceived and the behavior which may or may not result from that perception. This distinction is favored by experiments showing that pain reaction threshold, as measured by GSR, may vary independently of pain perception threshold, as measured by verbal report, under the influence of analgesics or spontaneously (**94**, **97**; see also **12**, **67**, **83**).

Skin Pain–Visceral Pain. Within the field of pain perception, it is usual to distinguish between pain coming from the receptors in the skin and pain coming from muscles or viscera. Most experimentation has been done on the former, since the skin is more accessible to easily controlled stimuli. However, clinical experience suggests that visceral pain is the more important of the two in the life history of most persons. This distinction as to origin does not necessarily imply a similar distinction as to quality; whether visceral pain is qualitatively the same as skin pain is a disputed question (**18**).

First Pain–Second Pain. When the skin is pricked, there will sometimes be a distinct double experience of pain. The first pain is usually described as sharp and pricking; the second is a longer, more burning pain. This qualitative distinction between first and second pain is also in dispute. In general, only the extremities show this double pain.

Pricking Pain–Burning Pain. The distinction between pricking and burning pain is more general than the first pain-second pain distinction. Pricking pain is, in general, reported for intact skin, while skin in which nerves are regenerating, which has been partially dener-

vated, or which has been injured so as to cause hyperalgesia yields burning pain on appropriate stimulation. Visceral pains have been called similar to burning pain. It is a matter of controversy whether or not these are differences in anything more than length of the experience (**2, 48, 87**).

Epicritic Sensibility–Protopathic Sensibility. Henry Head (**36**) in 1905 used these terms to describe the results of his experimentation on cutaneous nerve regeneration. Epicritic sensibility, the ordinary sensibility of the skin, is accurately localized and not painful. Protopathic sensibility is diffuse and unreasonably painful, has a higher threshold than epicritic to most forms of stimulation, and does not give experiences of touch. His theory was that these two forms of sensibility are mediated by two forms of innervation. The protopathic nerves are more primitive and regenerate more quickly than the epicritic, but are ordinarily inhibited by the epicritic nerves in the central nervous system. Nerve division at the periphery, the spinal cord, or the thalamus will stop the inhibitory function of the epicritic system in the areas affected, leaving the unpleasant protopathic system in action, if there is any effective innervation of the affected region at all. It is a question whether Head would have equated this distinction, so far as its reference to pain is concerned, with the distinction between pricking and burning pain discussed above. Most experimenters have failed to repeat Head's observations (**9, 79, 82**), so the concepts of epicritic and protopathic sensibility have fallen into disuse.

Neuralgia-Neuritis–Hyperalgesia-Causalgia. Neuralgia is a constant pain in the distribution of a particular sensory nerve, which may or may not be caused by neuritis, which is an inflammation of a nerve. Hyperalgesia is an increased sensitivity to painful stimuli in a restricted area. Causalgia is a syndrome which includes hyperalgesia and a constant burning pain. It is usually the result of a nerve lesion.

III. Pyschophysics of Pain

The subject of pain has been the province of the phenomenologist, the physiologist, and the physician, especially the latter. It is a difficult area in which to do experiments. Stimuli which will produce only pain are difficult or impossible to come by; the quantitative relationships between the stimulus as controlled by the experimenter and the effective stimulus at the receptor are unknown; and the range in which painful stimuli can be applied without causing excessive tissue damage is small. Subjects willing to expose themselves to painful stimuli may be hard to find. The responses used as indicative of pain are not always highly correlated, and any strong painful stimulus is likely to generate an emotional state which makes detached observation and report of the resulting pain difficult.

The literature to be reviewed here is mostly a medical literature. This means that it includes a good deal of case material and that the experimental studies frequently are inadequately controlled. Very few of the studies reviewed here reported standard deviations; even fewer made tests of significance. Clearly the quantification of our knowledge of pain has hardly begun. However, this should not be taken as adequate reason for discounting the value of the conclusions which medical experimenters or clinicians have drawn. The qualitative problems of pain are numerous and difficult, and in some of them the informed opinion of a doctor who has been treating patients in pain most of his life may be more useful than any experimental attack could be. In any case, until more carefully controlled studies are done and better methods of attacking the problems of pain are available, the information from these studies remains the best there is in the field.

In the last decade a wholehearted attack on the problem of quantifying the field of pain in a manner analogous to the psychophysics of vision or hearing has been made by J. D. Hardy, H. G. Wolff, H. Goodell, and their associates at the New York Hospital–Cornell University Medical College. This whole program has been based on the experimental technique of measuring pain thresholds or other quantities by means of a projected beam of heat and light which stimulates a blackened area of the skin (**29**). The exposure was for three seconds (as a rule), and the intensity of the beam was controlled by a rheostat. In later experiments, a variac was used. Stimulus intensity was measured with a radiometer, or, in later experiments, with a scaled voltmeter calibrated to give equivalent measures. Subjects (usually the experimenters) were instructed that they would experience "warmth mounting into heat which would suddenly, just at the end of a given exposure, 'draw together' into a sharp prick or tingle (**2**)." The threshold was approached from below only, using a variation of the method of limits. The point at which the subject reported pain was taken as his threshold.

There are difficulties in this procedure, of course. It does not provide for the statistical manipulation of data which has been found necessary in the psychophysics of other sense modalities. The approach only from below is likely to introduce a constant error into threshold readings. The use of a radiometer reading as an index of the amount of thermal energy delivered at the skin depends on the thoroughness and constancy from person to person of the blacking of the skin. Most important, Lewis (**48**) has shown that radiation applied to the skin will produce hyperalgesia, due to the release of histamine (**65**) or a histamine-like chemical (**48**) into the skin. Schumacher (**68**), using the Hardy-Wolff-Goodell apparatus, has shown the same thing for ultraviolet radiation. It is possible that the weak stimuli used in threshold deter-

mination do not have this effect; but if they do, the measures which Hardy, Wolff, and Goodell have made must have been seriously affected. The earlier papers are not explicit as to where successive measurements were made, but every description given suggests that successive measurements were made at the same place on the skin. It is only in a paper published in 1949 (**27**) that the first warning appears not to present successive stimuli to the same area of skin. (This warning is in connection with suprathreshold stimuli, but previous papers [**32, 33**] also used suprathreshold intensities, without warning. In these previous papers it was recommended that exposures be spaced out: five stimuli in ten minutes in the first of them, and five minutes between stimuli in the other.) Finally, none of the papers reports tests of significance to justify conclusions, and few of them report variability data in such a form as to make such tests possible. All these experimental difficulties should, of course, be corrected if possible in future experimentation, but the results reported by Hardy, Wolff, and Goodell are apparently fairly stable and reproducible (**12, 73**) and it seems unlikely that their more important conclusions will be changed too much by future studies.

In their original paper, published in 1940, Hardy, Wolff, and Goodell (**29**) reported that the pain threshold for their own skins was approximately 0.23 g. cal. per sec. per sq. cm. of skin surface stimulated. Pain sensitivity was separable from touch, heat, and cold, in that stopping circulation in an arm destroyed touch, heat, and cold sensitivity after 25 min., while the pain threshold, though raised, was still measurable. Furthermore, it was demonstrated that pain elsewhere could raise the pain threshold as much as 35 per cent. The pain generated by stoppage of circulation of the arm was found to raise the pain threshold at the forehead by the same amount as the threshold at the arm was raised, suggesting that this rise in arm threshold was not due to the ischemia produced by circulation stoppage. Also, acetylsalicylic acid (aspirin) was shown to lower the warmth threshold and raise the pain threshold, indicating that the two are separate senses, although stimulated together in this technique. The size of the area of skin stimulated was shown not to influence the amount of the threshold, indicating that there is no spatial summation for pain (in contrast to heat, for which spatial summation is almost perfect [**28**]), and justifying the authors in reporting thresholds in terms of a physical unit independent of the amount of skin stimulated.[3]

[3] Gram calories per second per square centimeter of skin stimulated, the unit used, is actually independent of the amount of skin stimulated. As the amount of skin stimulated is increased, the intensity of the light remaining the same, the total energy in gram calories delivered to the skin will increase, since area increases, but the energy per unit of area per unit of time will remain the same.

Following this basic paper, Wolff, Hardy, and Goodell reported a series of studies on the effects of various analgesics on the pain threshold (93, 94, 95, 96, 97). The principal findings were:

1. Most ordinary analgesics raise the pain threshold (94, 95).
2. The extent of the rise depends on the amount given, but there is a maximum rise which any given analgesic can produce. This maximum rise may be as much as 100 per cent (94, 95).
3. Intake of quantities greater than adequate to produce maximum rise will in general prolong the effect of the analgesic (94, 95).
4. The pain-threshold-raising properties of analgesics given in combination do not summate; such a combination is only as effective as its most effective ingredient (94, 95).
5. The opiates (94), ethyl alcohol (97), and to a lesser extent other analgesics have the additional effect of dissociating pain perception from pain reaction. Subjects may perceive considerable pain, but do not react to it. This effect does summate and has no observed limit.
6. If serious pain has existed for a while before administration of an opiate, such administration will not affect the pain threshold, but will affect the pain reaction (94).
7. It is concluded that the reduction or removal of the pain reaction is the main value of opiates, and is the most important effect for which doctors administering analgesics should try (94).

The results of these studies, however, are called into question by another study by Wolff and Goodell (91), in which subjects were administered placebos. The results were that, in a suggestible subject, a placebo believed to be an analgesic will raise the pain threshold while an analgesic believed to be a placebo will have no effect on the pain threshold. (The latter conclusion was demonstrated only for acetylsalicylic acid; no other analgesics were tried.) No data were given as to whether or not the subjects of the previous experiments on real analgesics were suggestible.

So far, all the studies described have exposed the stimulus for three seconds and instructed the subjects to report the threshold of pricking pain. Bigelow, Harrison, Goodell and Wolff (2) tried to compare the thresholds for pricking and burning pain under various conditions. They found that the threshold for burning pain was lower than that for pricking pain. There are, however, two experimental difficulties. First, the differences as graphed are so small that a test of significance is needed to determine whether or not they are random, particularly since the data for only eleven of the twenty-three subjects used are included in the graph from which the conclusion cited was drawn. Furthermore, the authors reported that it was easier to judge the pricking pain threshold at the shorter exposure lengths, while longer exposure lengths were best for the burning pain threshold. This leaves some doubt as to

the validity of reports of both measures for the same exposure time. This doubt is strengthened by Lewis's (**47**) contention that actually the only difference between pricking and burning pain is the length of stimulation, a view which Bigelow, Harrison, Goodell, and Wolff do not share. It is not agreed whether the first of the two cutaneous pain responses from an extremity is pricking and the second burning pain, so Bigelow, Harrison, Goodell, and Wolff tried to settle the issue by asking nineteen subjects. The subjects disagreed: eight said the two pains were of the same quality, nine said they were different, and two were undecided. The authors also measured the two thresholds on the glans penis, which Head (**36**) had considered to have only protopathic sensibility. Here the two thresholds were widely different, with the pricking pain higher. The introspective reports, however, leave much doubt as to whether the subjects were judging the same kind of pricking pain in this case as they were in measurements on the arm or forehead, as the authors point out. The authors report several other experiments involving ischemia, which abolishes the first pain, or procaine nerve block, which abolishes the second. In all of these, the first pain was felt as pricking and the second as burning. However, the duration of pain was uncontrolled for all of these cases.

Hardy, Wolff, and Goodell (**31**) proceeded in 1947 to the next step in the psychophysics of pain—the measurement of the just noticeable difference. They found that the ratio of stimulus intensity at threshold to stimulus intensity adequate to produce tissue damage was only 1:2, which they interpreted in terms of the biological inefficiency of a pain sensitivity which would extend much farther below the point of tissue damage. Within this range, they reported that there were 21 j.n.d.'s. The size of the j.n.d. changed, but was approximately 0.01 g. cal. per sec. per sq. cm. in the lower ranges of stimulation. The method used was again a variation of the method of limits, open to the same criticisms discussed above.

Now the ground was clear for the construction of a scale of pain intensity, which the next paper (**32**) did. This paper determined the j.n.d. at various intensities from threshold to the point at which changes in intensity could no longer be discriminated, and then used these j.n.d.'s to compute a double j.n.d. scale. The units of the scale were named *dols*, and the range of the scale was, of course, 10½ dols (corresponding to 21 j.n.d.'s). The values of the j.n.d. used in calculating the scale were, however, adjusted somewhat from the original data. How much difference this adjustment makes is not clear.

A follow-up study (**33**) attempted to valiate this scale by the method of fractionation. The validation was carried out both on the

authors and on seventy medical students. The authors assert that the validation is close, but failure to report variability data or to test significance makes it impossible to evaluate this conclusion. The average figures presented look like good validation.

Finally, Hardy and Javert (**27**) tested the usefulness of the dol scale by measuring the pain intensity of childbirth, using estimates of absolute intensity in dols. The only theoretically important conclusion was that once again the distinction between pain perception and pain reaction was necessary to make sense out of the results. The maximum pain perceived ranged from about 7 dols (without anesthesia) to 10½ dols, the top of the scale.

This is the most recent in a long series of related experiments by the same group, using the same apparatus, and directed towards the same end: the quantitative examination of pain perception. The series, taken as a whole, seems to offer three conclusions:

1. It is both possible and desirable to work out the psychophysics of pain.
2. A distinction between pain perception and pain reaction is probably necessary in order to organize the experimental results in any meaningful way.
3. As the psychophysics of pain progresses, it will become increasingly necessary for experimenters to keep pace with the quantitativeness of their problems by refinement of their experimental and statistical procedures.

A number of other techniques have been used, with varying degrees of success, in the attempt to measure pain thresholds (**8**, **24**, **35**, **73**). Slaughter and Wright (**73**) have modified the Hardy-Wolff-Goodell apparatus and procedure by the addition of a constant voltage regulator to avoid fluctuations in heat delivered due to fluctuations in line voltage, the use of a small thermocouple just in front of the subject's skin to measure strength of stimulus at the time the stimulus is applied (in contrast to the later use of a radiometer by Hardy, Wolff, and Goodell) and the reporting of results in degrees. The first two improvements seem desirable. The third is open to objection. The effective stimulus to pain in a situation in which radiant heat is applied to the skin is presumably heat transfer. Heat transfer is dependent on the amount of heat delivered to the skin, but only secondarily dependent on the temperature at which the heat is delivered. Actually, the two measurements should be convertible one into the other, since both are made before the stimulus reaches the subject. Using this technique, Slaughter and Wright (**73**) have found much more inter-person variation in pain threshold than did Hardy, Wolff, and Goodell. Leduc and Slaughter (**46**) have in general confirmed the Hardy-Wolff-Goodell results with opiates.

Goetzl, Burrill, and Ivy (**24**), after an extensive review of the algesi-

metric literature, proposed (in 1943) another method which they considered more desirable than any proposed up to that time. This method consisted of applying pulses of alternating current to a tooth. The intensity and frequency of occurrence of the pulses could be varied. The procedure was used on men and on dogs. On men, the current was applied through a platinum electrode into a filling, and the other electrode was a bath of salt water into which the hand was put. On dogs, two holes were drilled, one on each side of a tooth, and the holes were filled with fillings especially hollowed out to receive the electrodes. In this case no indifferent electrode was used; the current was passed from one side of the tooth to the other. A reflex contraction of the facial muscles was considered the sign of pain. Using this technique, Ivy, Goetzl, Harris, and Burrill (**38**) showed that epinephrine has analgesic properties, and Parsons and Goetzl (**57**) and Goetzl (**23**) tested the effects of acetylsalicylic acid and other antipyretic drugs on the pain threshold. They concluded that antipyretic drugs do not have pain-threshold-raising properties. Parsons and Goetzl (**56**) also confirmed the observation by Hardy, Wolff, and Goodell (**29**) that the presence of one pain raises the threshold for another. Robertson, Goodell, and Wolff (**64**) later used a modification of Goetzl, Burrill, and Ivy's technique and found considerable variation in threshold value observed depending on placement of electrodes, recent stimulation, etc. It is not surprising that such a technique yields variable results; the resistance of the tooth to the passage of current is certainly likely to be important in determining the amount of current necessary to produce a report of pain; this resistance is very likely to vary from subject to subject, and maybe even from time to time for one subject. On the other hand, this technique is obviously superior to the Hardy-Wolff-Goodell technique for animal experiments, since it requires no particular cooperation from the animal. It is probably the best technique yet available for pain experiments on animals.

Lewis (**48**) has proposed and Harrison and Bigelow (**35**) have systematically used a technique for measuring pain threshold in ischemic arm muscle. The circulation of the arm is blocked, and the subject makes a series of hand contractions of constant size, frequency, or energy. The number of contractions needed to elicit pain is used as the threshold figure. It is necessary, of course, to allow enough time between successive threshold determinations for dissipation of the chemical which causes ischemic pain (**48**), and to avoid muscle fatigue, which raises the threshold (**35**). With these precautions, this method yields highly constant thresholds, which are amenable to experimental alteration in a manner analogous to the experiments of Hardy, Wolff,

and Goodell and which are not subject to the difficulties which repeated exposures to radiant heat present. The main advantage of this technique, however, is that it permits a better determination of the endpoint for pain. So long as the muscle is not used, whatever degree of pain has been developed in it remains fairly constant. This makes possible relatively leisurely observation of the experience and so presumably an easier judgment as to whether or not pain is actually present.

The experiment of Denton and Beecher (**15**) is unique in the pain literature for its statistical sophistication (the authors give credit to a mathematical statistician, Dr. F. C. Mosteller, for statistical advice). This is an elaborate experiment attempting to compare the analgesic and other effects of several drugs. In most respects it is a model of experimental design. The pain used, however, was pain resulting from many different kinds of surgery, and the comparison of analgesic effects naturally becomes less convincing because of the variation in amount and kind of pain which the analgesic was used to control. The experimenters tried to randomize administration of each analgesic for these variables, but it is impossible to tell whether this was successful or not. The fact that 429 patients were used, randomized for type of operation over three types, suggests that they may have been successful. In this experiment, no pain thresholds were measured; all measures were of the amount of analgesic required to give relief from pain to 90 per cent of patients receiving it. A parallel experiment performed on healthy subjects was designed to determine the side-effects of the drugs. Among other procedures, the Hardy-Wolff-Goodell technique was used, but the results were so unsatisfactory that they were not even reported. For example, a salt solution frequently showed as much analgesic effect as morphine, or more.

IV. Skin Pain Receptors and Afferent Neurons

Writers on the receptors and afferent nerves involved in pain have centered on three main problems. First, there is the old von Frey-Goldscheider controversy as to whether pain has separate receptors. This issue has been almost completely settled in favor of von Frey and separate receptors. (For a discussion of the evidence leading to this conclusion, see Morgan [**55**].) If more evidence were needed, the dissociation of pain thresholds from those of other kinds of cutaneous sensibility demonstrated by Hardy, Wolff, and Goodell (discussed above) would serve. Not everyone is satisfied, however; Hebb (**37**) has argued very recently for a return to the intense-stimulation theory. His views

will be discussed later in connection with the problem of brain functioning in pain.

The second problem with regard to skin pain receptors is raised by the double pain experience mentioned above. In general, this double pain from a single stimulus can be excited only on the extremities, which has led to the theory that it represents the different conduction times of different pain nerves en route to the central nervous system (**10, 22, 48**). Lewis (**48**) and Gasser (**22**) have each summarized the evidence for this interpretation, which is mostly derived from the results of different techniques of anesthetization of nerve fibers. Thus, Gasser (**22**) has shown that cocainization abolishes first the response of slow unmyelinated fibers. Correspondingly, Lewis (**48**) has shown that cocainization either of skin or of the afferent nerve first abolishes the second pain response. Gasser also demonstrated that ischemia abolishes the response of fast fibers, and Lewis (**48**) and Zotterman (**101**) have shown that it abolishes the first pain response. However, the evidence is not as yet complete; the quantitative temporal relations of ischemia data conflict as between Lewis and Zotterman, and no one has combined ischemia or cocainization with recording of the nerve impulses. The technique of Rusinov (**66**) for recording pain impulses should be adaptable to this problem.

The third issue to be considered here is that of the nature, distribution, and functioning of the end organ for pain. Here, too, the evidence is pretty clear. Four different histological investigations of various skin areas have been made, and all agree as to the nature of the end organ for pain (**6, 78, 85, 99**). Tower (**78**) examined histologically the cornea of the cat (the cornea is believed to be sensitive only to pain) and found "plexiform arrangements of nerve fibers . . . , bearing free nerve terminals in the form of simple terminal twigs, end knobs, loops, brushes, or minute skeins. . . . The terminal portions of this ramification are in all sites unmyelinated, but farther back, myelinated or unmyelinated fibers may enter into it (p. 17)." Such structures are found throughout the cutaneous surfaces, both within and beneath the epidermis. In general, the epidermal structures feed into fast, myelinated fibers, while the subepidermal ones feed into slower fibers.

The most important part of this conclusion is that such structures overlap and are widely ramified; in the cornea of the cat one fiber may receive impulses from stimulation of as much as one quarter of the whole cornea (**78**). This raises a serious problem: how is localization of pain as accurate as it is, and how do we get the so-called pain spots? Long ago, the pain spots were recognized as varying, and this picture sug-

gests the view that they merely represent accidental variation in the density of receptors stimulated. The problem of localization is more difficult, but the anatomical evidence forces the conclusion proposed by Tower (78) that the pattern of stimulation in overlapping pain receptor units feeding into different nerve fibers is "interpreted" by the brain in terms of localization. As Wolff and Hardy (92) put it: "The painful spot experienced as such when an area of normal skin is exposed to noxious stimulation is a centrally integrated experience projected onto the periphery for the specific purpose of localization. The 'skin spot' is, so to speak, a 'mind spot' (p. 168)."

Evidence in favor of this view, which has the added advantage of settling the problem of sensation during regeneration after nerve division, is offered by Weddell, Sinclair, and Feindel (86). These investigators made histological examination of partially denervated skin after degeneration due to nerve section. They found that when the area of skin stimulated had isolated nerve nets, the characteristic slow, excessively unpleasant, poorly localized pain observed during nerve regeneration was elicited, but that when the area had many interweaving nerve nets and terminals, as in normal human skin, normal pain perception was elicited. This experiment accounts for all of the facts of nerve regeneration except the original ring of hyperalgesic tissue at the periphery of the dead area immediately after the section of the nerve. This ring of tisue is accounted for by the observation by Weddell (85) of overlapping innervation of any one skin area by many fibers from different nerves. If the fibers from one nerve degenerate, this leaves the fibers from the other nerves innervating the same area of skin alone, which is the condition prescribed by Weddell, Sinclair, and Feindel for the hyperalgesic response.

The problem of the end organ for pain, and the related problem of whether or not there are different qualities of pain, is neatly rounded off by an observation by Feindel, Weddell, and Sinclair (18). They made histological studies of various deep pain-sensitive tissues, and found in every case the same pattern of isolated nerve nets found in partially denervated skin tissue. This accounts both for the relative insensibility of deep structures to ordinary stimuli and for the excessively unpleasant nature of visceral and deep pain, when aroused.

The research discussed in this section, taken all together, justifies the conclusion that there is fundamentally only one kind of pain receptor, which gives ordinary pain sensibility when enough receptors are stimulated, but the kind of peculiarly unpleasant pain found in partially denervated tissues when too few are stimulated. It is a high density

of these receptors, discharging into different nerves, which accounts for a "pain spot."

V. Disorders of Pain Perception

Aside from the problems of psychophysics and end-organ physiology, knowledge about pain is spotty and confusing. The observations of Weddell, Sinclair, and Feindel, cited above, suggest that much of what is important in the perception of pain goes on in the central nervous system. All that is known about disorders of pain perception heightens this belief. The history of experimentation in vision has been a history of experimentation on the peripheral mechanisms giving way to experimentation on brain mechanisms, as it became clearer and clearer that what happened in the eye was only a part of the whole problem of vision. In hearing, the problem of the role of the central nervous system is still more acute, but the experimentation has not for the most part been done. In pain perception (and, presumably, the skin senses in general) knowledge has hardly advanced beyond the point of asking questions about what happens in the central nervous system. The most important conclusion which can be drawn from the facts about pain and from the history of research in other sense modalities is that the picture will never be clear until the functions of the central nervous system in pain perception are well understood. The following discussion of abnormalities in pain perception illustrates this point.

The pathways which pain fibers follow are fairly clear as far as the thalamus. Skin pain fibers in general are conveyed to the dorsal roots by somatic nerves. Deep pain fibers may attach themselves to blood vessels for a while and then join autonomic nerves, may join autonomic nerves early and remain with them almost up to the cord, or may travel by somatic nerves. It does not seem to make any difference to the quality of the pain which path is followed. On entering the cord, all pain impulses are transferred to the opposite side and travel up the cord in the anterolateral section. From here, the path is uninterrupted for most pain impulses to the nucleus centralis posterior of the thalamus. Whether or not pain impulses are further projected to the post-central gyrus is uncertain; Penfield and Boldrey (**59**) were unable to elicit pain by stimulation of this area and hence concluded that pain is primarily represented in the thalamus, but analgesia and hyperalgesia have been reported in clinical cases involving cortical lesions (**53, 81**). Hemispherectomy does not result in any significant loss in pain sensibility on the affected side, but this operation apparently results in extensive thalamic degeneration also (**81**). The retention of sensibility suggests

a pathway to the ipsilateral side of the cortex. Also, a few cases involving very extensive thalamic destruction still show pain sensibility. In short, brain localization of pain functions is still very much an open question.

Probably the most interesting and dramatic type of disorder of pain perception is known as causalgia. Livingston (**50**) has described a large number of cases of this remarkable syndrome, enough so that certain constant features stand out. The initiating factor may be an injury of as traumatic a nature as an amputated limb, but lesions as small as a thorn prick have been demonstrated to produce causalgia, and a number of cases are available where no lesion has been found. The symptoms are: incessant burning pain from the affected region, extreme hyperalgesia from the skin in that region, lowered temperature in that region, relatively small areas showing even greater sensitivity than the "normally" hypersensitive area, muscle spasm (in some cases), excessive sweating in the affected region, degenerative tissue changes, and marked personality changes.

A number of these symptoms reflect the hyperactivity of the sympathetic nervous system that seems to be a standard feature of all cases of intractable pain. Leriche (**47**) has based his whole conception of the treatment of intractable pain on the concept of sympathetic involvement, a view which seems more plausible when it is remembered that many afferent pain fibers pass through sympathetic tracts (**3**). Another important feature is the presence of muscle spasm. It has been suggested that in many cases of intractable pain the compression of nerve trunks by spasm of adjacent muscles (or viscera) is a causative factor (**72**).

More significant than the nature of the syndrome are the techniques effective in curing it. Occasionally cutting of the pain pathway involved at the peripheral or spinal levels will be adequate, but more often such a procedure will fail. The most dramatic technique is the injection of novocaine into a supersensitive point in skin or muscle or a sympathetic ganglion. This may relieve pain for a short while, or, more dramatically, it may relieve the pain permanently (**26**, **50**). Such results, of course, suggest that some recurrent circuit mechanism was active, which once stopped did not start up again. Livingston's most dramatic case, making a very forceful argument for this interpretation, was of a man who developed causalgia of the left foot after a fracture. The middle three toes were flexed in spasm. After a few years, the corresponding three toes on the right foot began to flex, and simultaneously the right foot, which had not been injured, became slightly causalgic.

Amputation of the affected toes of both feet left very painful phantom toes on the left foot, which were eventually cured by amputation of that foot. Here the spread of the symptoms to the right foot after injury and prolonged pain in the left foot clearly suggests some active process in the central nervous system, which gradually recruited more and more of the fibers in that area into its activity.

A particular kind of causalgia which is very informative because it is so frequent is that resulting from limb amputation (**63**). Such amputations almost invariably leave phantom limbs, which feel to the patient just like real limbs. In non-painful cases the phantom limb can be moved and used, feeling just like a real limb. But in the very common painful phantom limbs, the phantom is usually tightly flexed, immobile and very painful. Such causalgias may be caused by neuromas in the stump, in which case excision of the neuroma may cure the pain. But frequently there is nothing wrong with the stump, and sectioning of the nerve trunks innervating it does not relieve the pain. Here again, anesthetization or destruction of sympathetic ganglia may result in permanent or temporary remittence from pain. It is interesting that along with the remittence from pain the patient regains the ability to manipulate his phantom limb. A frequent observation is that a resection of a nerve serving the stump may yield remittence for a while, with subsequent return of the pain. Whether this is due to growth of adjacent nerves into the denervated area or whether it is due to perpetuation of the central pain process has not been determined.

That these results are not due to psychogenic factors is suggested by Livingston's observation that injection of saline salution into a hypersensitive "trigger point" or a sympathetic ganglion has no effect, while injection of novocaine may have the dramatic effects described above. Livingston (**50**), from whose treatment and Riddoch's (**63**) the above picture was drawn, interprets the phenomena of causalgia as follows:

> An organic lesion at the periphery, involving sensory nerve filaments, may become a source of chronic irritation. Afferent impulses from this "trigger point" eventually create an abnormal state of activity in the internuncial neuron centers of the spinal cord gray matter. The internuncial disturbance in turn is reflected in an abnormal motor response from both the lateral and anterior horn neurons of one or more segments of the cord. The muscle spasm, vasomotor changes, and other effects which this central perturbation of function brings about in the peripheral tissues, may furnish new sources for pain and new reflexes. A vicious circle of activity is created. If the trigger point is removed early, the process may subside spontaneously. If the process is permitted to continue, it spreads to involve new areas, and tends to acquire a momentum that is increasingly difficult to displace. Perhaps in this stage even

a removal of the original irritant may not be sufficient to establish a cure. But if an important part of the circle of reflexes can be interrupted, the process may subside, and a normal physiologic status is again established (**50**, pp. 226–227).

This view is a far cry from a simple one-receptor–one-sensation view of pain. And it is not at all unique to investigators in this field. Russian investigators since the war, operating on a similar hypothesis, have found that up to one hundred hours of continuous sleep is an excellent treatment of causalgia (though they were working only on newly injured cases [**39**, **100**]). They have also had good results with novocaine block of the affected nerve (**80**). Cohen (**13**) has produced the pain of angina by application of an irritating plaster to skin innervated by the same segment as the heart in patients with heart disease, a result which he interprepts in terms of summation of subliminal pain impulses in the central nervous system. Bishop (**4**), from his experiments on electrical stimulation of the skin, concludes:

> Even though electrical stimulation of one ending of one fiber may be possible, it does not follow that the central nervous system transmits and interprets the impulses induced in a correspondingly simple manner. The chief virtue of such experimental restriction of activity to one peripheral element may be the demonstration that such stimulation is insufficient to enable the central nervous system to obtain an adequate estimate of external conditions. It may be inferred that the central nervous system is designed not primarily to detect discretely the activity of each ending, but rather to integrate the activities of many; and even peripheral sense organ activity will not be satisfactorily analysed, at least in terms of a subjective sensory response, without a further understanding of the central mechanisms that react to it, as well as a knowledge of what each sense organ does to its immediate nerve fiber (**4**, p. 379).

A variety of evidence about this notion of a spinal interaction among pain processes is available, most of it favorable. For example, Kunkle, Charles, Armistead, and Goodell (**45**) have shown that pain produced by immersion of one finger in cold water spread to other fingers, but that pain did not so spread from the thumb to the jaw (whose representations in the post-central gyrus of the cortex are adjacent), and so concluded that the neural process involved is in the spinal cord. Thompson and Gellhorn (**77**) have shown that painful stimuli can produce spinal reflexes in spinal animals. On the other hand, Bingham (**3**) argues that the effectiveness of anesthetization or destruction of sympathetic ganglia in curing causalgias is the result of blocking of sympathetic afferents and not of stopping the support of some ongoing spinal activity; and Doupe, Cullen, and Chance (**16**) believe that causalgia is caused by direct stimulation of pain afferents by sympathetic efferents.

Aside from the recruitment and interaction effects postulated above, there is also the concept of inhibition. The view of Head (36) was that epicritic fibers inhibited protopathic, and this view is still widely held in one form or another. The most common view is that the impulses resulting in fast pain modify the central nervous system in such a way as to prepare for the subsequent impulses resulting in slow pain (42).

The only detailed theory of the functioning of the central nervous system in pain has been offered by Hebb (37). His theory of pain is interwoven with the rest of his theory of brain function, but it can be summarized by saying that he conceives of pain as being a massive disorganized firing of thalamic or cortical neurons. This massive, disorganized firing may develop if too few impulses from C fibers reach the central nervous system, allowing localized hypersynchrony of firing to take place in the thalamus. It may also take place if impulses along C fibers arrive so frequently as to dominate local thalamic action, again producing localized hypersynchronous firing. In the intermediate normal range of action of C fibers, their effect is perceived as touch. To account for the fact that injury of the somesthetic system anywhere from peripheral nerve to thalamus may produce pain, and to account for the nerve division experiments of Head (36), Boring (9), and others, Hebb postulates that hypofunction of the somesthetic system leads to pain. He assumes that more general hypersynchronous firing, or more generally organized firing of any kind, will not be accompanied by the experience of pain; only local hypersynchrony is painful. The reason why local hypersynchrony may develop is that in the absence of the arrival of continuous normal somesthetic impulses, "Higher somesthetic nuclei then lack their normal degree of sensory control and become hyperresponsive, and yet are still exposed to an intermittent 'triggering' from peripheral stimulation" (37, p. 188).

The notion of hypofunction as a cause of pain is a new and imaginative one, but it raises the question why operations attempting to cure intractable pain by severing the pain pathways, either peripherally or in the spinal cord, are ever successful. Diseases such as syringomyelia, in which afferent pathways degenerate without leaving the patient in pain, are also unexplained by Hebb's theory, as he has pointed out. It is probable that the usefulness of Hebb's theory of pain will depend on the usefulness of his view of the function of the central nervous system.

It would be misleading to leave the topic of central nervous system functions in pain disorders without considering some of the disorders which are or may be peripheral in origin and cause. Of course, neuralgia due to neuritis is presumably peripheral in origin. Lewis (48) has performed a series of experiments showing that hyperalgesia of the skin

such as is produced by sunburn is caused by the release of a chemical (histamine-like, for Lewis, but Rosenthal and Sonnenschein [65] argue that it actually is histamine) into the skin. Such hyperalgesia occurs in the area of distribution of the nerve innervating the area damaged, when it results from localized damage (e.g., that due to repeated strong electrical stimulation of a small area). Lewis blocked a nerve with novocaine and then stimulated the nerve trunk either above or below the block. If the stimulation was applied above the block, pain was felt, but no hyperalgesia resulted. If it was applied below the block, no pain was felt, but hyperalgesia resulted after the block wore off. Lewis concluded that the effect depends on the spread of nerve activity, and that what happens is that noxious stimulation causes the nerves stimulated to release the histamine-like chemical in the surrounding area. He attributed all this to a special set of nerves which he called the *nocifensors*, and which have no functions other than those described above. He postulated these nerves because the facts of localization seemed to him to require punctate distribution of pain receptors and neurons, which is not consistent with the diffuse distributions shown by the hyperalgesia. However, the more recent work on localization and pain spots described above disposes of this problem, and leaves it as probable that Lewis's results are caused by antidromic impulses in the sensory fibers of the pain system—an interpretation accepted by many of the more recent writers on the problem (**78, 87**). This looks like a clear demonstration of a peripheral mechanism for one kind of hyperalgesia, but it may not be. In 1950, Hardy, Wolff, and Goodell (**34**) published a series of observations on hyperalgesia which they feel must be explained in terms of an ongoing facilitative activity in the spinal cord, of much the same type as that described above by Livingston. They found, for example, that spatial summation of pain impulses occurs in hyperalgesic skin but not in normal skin. It remains to be seen whether the peripheral theory of hyperalgesia can be extended to cover their observations.

Another phenomenon which unfortunately has not been adequately examined, is the cases of people who are congenitally insensitive to pain. That such cases exist in very clear (**19, 44**). No histological studies of the skins of such people have been reported. But the presence of spinal reflexes which are assumed to be initiated by pain impulses suggests that the defect is in the central nervous system.

A good deal of research has been done on the mechanisms underlying various other kinds of pains, especially visceral pains (**35, 52, 87**) and headaches (**51, 70, 71, 88, 90**). This work will not be reviewed here, since it has contributed nothing new to the psychology of pain in general,

aside from a demonstration of the great frequency of inaccurately localized or "referred" visceral pains. Discussions are also available of the various surgical techniques for curing intractable pain (**20, 47, 76**) including particularly cutting of the posterior roots (**60**) or of the anterolateral tract (**75**). Recently a number of surgeons have begun to perform prefrontal lobotomies to cure intractable pain (**17, 83**). One interesting result of this is that a patient who formerly was completely dominated by his pain will after operation show no pain symptoms and be able to go about his business with no impairment of function due to pain; but if he is asked whether he is still in pain, he will report that it is just as bad as ever, but that he really doesn't care. More evidence for dissociation of pain perception and pain reaction!

VI. Summary and Conclusions

From consideration of the nature of pain and a review of the literature, four main conclusions have emerged.

1. No operational definition of pain is at present available. The literature on pain has defined pain introspectively, and so far the definition has been quite satisfactory.

2. The psychophysics of pain has been well advanced as a result of sustained experimental attack in the last decade, but its further development depends on the correction of experimental errors and the use of more adequate statistical tools.

3. The localization of pain stimulation on the skin is a function of presentation to the central nervous system of a complex pattern of impulses from many different pain fibers of varying transmission speeds. Disturbances of this pattern will result in hyperalgesias and other disorders of pain perception. Qualitative differences in pain perceptions from different parts of the body are probably due only to differences in the pattern of impulses in the central nervous system resulting from differences in density of peripheral innervation.

4. Attention to the peripheral mechanisms of pain should not preclude recognition of the vital role played by the central nervous system in pain perception. In many cases, knowledge of what is happening at the periphery is inadequate for or unrelated to prediction of the pain experience. Study of cases of disorders of pain perception, especially causalgias, suggests that among the important processes going on in the spinal cord are (a) recruitment of fibers into an ongoing activity of internuncial neurons, either sustained by or independent of peripheral stimulation, (b) a vicious circle of pain-induced efferent activities resulting in sympathetic and somatic changes which accentuate the original pain stimulus, and (c) inhibition of pain by pain, pain by other skin senses, and possibly pain by the cortex.

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STATISTICAL TECHNIQUES IN PRESENT-DAY PSYCHODIAGNOSTICS[1]

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More and more the clinical psychologist is submitting the data he gathers to rigorous examination, applying the experimental principles of research design and statistical treatment to his everyday observations as well as to research material. This emphasis in present-day clinical psychology was well reflected in the symposium on Statistics for the Clinician (29) introduced by Zubin and presented to the Biometrics Section of the American Statistical Association. At that time a series of clinical problems was proposed by A. L. Baldwin, J. F. Kubis, L. S. Kogan and J. McV. Hunt, A. I. Rabin, and L. J. Cronbach. Solutions to the problems were suggested by W. Stephenson, E. F. Gardner, D. Horn, H. Guetzkow, and G. W. Brown. The present survey is much on the order of that symposium in reviewing the problems of statistical techniques in present-day psychodiagnostics and indicating the solutions attempted—whether proposed, planned, in progress, or completed.

The problems of validity, reliability, and other statistical treatment of data obtained in psychological practice have accompanied the use of diagnostic devices since their inception. As the projective methods became a more prominent tool of the clinical psychologist, still more statistical problems were posed because of the nature of the tests and the data obtained through their use. Within the past few months Cronbach (6) has reviewed the special area of the Rorschach method and presented evidence of the clinician's concern with "statistical confirmation for their hypotheses." The errors, omissions, and "unhappy choices of statistical procedures" were pointed out and suggestions made for more adequate statistical treatment. However, it is not only in the area of projective techniques that statistical questions have been raised; for these questions apply also to other psychodiagnostic devices. Loevinger (14) has pointed out some of the problems in scoring non-projective psychological tests of abilities, attitudes, and personality traits.

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This report was presented in brief as a cross-committee study with the Research Committee of the Division, Dr. R. E. Harris, Chairman, at the 1949 American Psychological Association Convention, Denver, Colorado.

Cronbach recently outlined the area of diagnostic devices which is the concern of the present survey:

> In the past two decades, attention has been given principally to developing a workable tentative theory of personality and inventing promising diagnostic methods. Until one has materials worth validating, validation research must hold a secondary place. We now have projective techniques, situational tests, and numerous other new tools which purport to describe personality; there lies ahead a large chore of validation to define the limits of dependability of each technique (4, p. 373).

More specifically, the major emphasis in statistical treatment of data from the psychodiagnostic devices has shifted from "group-centered" to "individual-centered" statistics (27). This shift appears in the suggestions of Cattell (3), Horn (12), Stephenson (21, 22), Zubin (26, 27) and others. Nor is this entirely a new development, for a decade ago Woodworth (24) wrote of the need to combine statistical analysis with individual study, and of the advantages and disadvantages of the two approaches when applied separately. Even earlier, Allport (1) discussed "the study of general principles and the study of the individual case" and the philosopher Windelband's proposal to separate the *nomothetic* from the *idiographic* disciplines. Allport at that time pointed out that psychology "in the main has been striving to make of itself a completely nomothetic discipline," but cautioned that the two approaches should be regarded as overlapping and as contributing to one another.

Zubin (27) felt that the recent symposium on Statistics for the Clinician was, in fact, "an attempt at quantifying the idiographic approach," but recognized that in present-day practice the entire emphasis is not on the individual case, because in that symposium "not all the enthusiasm of the present writer for 'individual' statistics is shared by the other contributors." Yet the stress on the individual case and on "intra-individual" study is very much evident in current literature. Why the trend toward individual study, particularly by the clinician?

Basically and ultimately, of course, the clinician is concerned with the individual case, for the single person is the unit in psychodiagnostics, in therapy, and in research, no matter how many units are studied. But the problem goes beyond this, and with the exception of relatively recently developed statistical techniques there have been no other than the methods based on the assumptions of normal distribution of a series of cases. This does not mean, however, that the "normal law of error" of Laplace, Gauss, Quetelet, and Galton has gone unchallenged. Particularly since the introduction of the projective methods have

questions been raised about the normal distribution curve and its application to data obtained by means of such psychodiagnostic devices. In the early days of the use of the Rorschach method in this country, Meltzer (16) noted that "the habit of regarding the normal distribution curve as a point of reference" meant that the psychologist saw test results which deviated from the normal curve as "indicative of a poorly constructed test." Meltzer emphasized that it was not necessary to assume that all measures of human nature must yield normal distributions, and cited the discussions in E. L. Thorndike's *Mental and Social Measurements* and T. L. Kelley's *Statistical Methods* in evidence.

Recently this argument again has been advanced by a number of investigators. Rabin (18) summarized, as one of the statistical problems faced in Rorschach analysis, that there are "*n* factors which are not similarly distributed; some follow a normal distribution, some a *J*-curve and some other forms not yet investigated." Gardner (9) also has written that there are "many situations in which the assumption of normality seems unwarranted . . . it often seems more reasonable to expect a skewed distribution rather than a normal distribution. . . . "

Finally, many students of the problems of statistical methods for data from psychodiagnostic devices are now proposing re-evaluation of our thinking in this area. These proposals principally have concerned the projective techniques, although the more traditional devices also have been re-examined in this light. Loevinger has summarized the trend:

> In the field of measuring personality characteristics, there has been far from unanimous agreement as to the value of traditional psychometrics, and some of the most promising tests, notably the projective tests, have largely bypassed traditional procedures. Even with reference to measuring abilities, the pages of *Psychometrika* have contained an increasing number of articles in the past few years exploring the contradictions and dilemmas which arise from application of the concept of reliability, of the method of rectilinear regression, and of heterogeneity of test content. . . . Development of factor analysis has sharpened the appreciation of these difficulties (14, p. 521).

Further, Symonds (23) prefaced his recent text on personality dynamics with a statement that the demand by the scientific method for the reproducibility of data cannot now be met in clinical practice. He emphasized the difficulties facing an attempt to "repeat all of the conditions which would be necessary" in order to obtain the same data in successive samples in psychotherapy for reliability and validity studies. Also, there are no absolute definitions of "normality and good adjustment," for the criteria vary according to the culture, the place, the

time, and the individual—"Each person makes the only adjustments possible for him. . . . " (23, p. 569). This is the philosophy of the ultimate in the idiographic approach. Frank's position has been similar in nature, and Cronbach (7) concludes that he "appears to argue that cases cannot be treated objectively, quantitatively, and anonymously, when projective tests are used." Yet, there are those, like Grant (10), who feel that the basic concepts are as appropriate for clinical data as for other material and that the problem is one of devising special tests and techniques for treating the obtained data. In reviewing "new procedures of experimental design and analysis," Grant finds many which already have proved useful for clinical problems. Moreover, it is his feeling that "The theory of experimental design has, at least temporarily, far outstripped psychological practice."

Proposals for Solving the Statistical Problems in Present-Day Psychodiagnostics

Zubin (27) already has summarized a number of the methods for treating data from diagnostic devices, including pattern analysis, factor analysis, other correlation techniques including both the traditional *R*- and the newer *Q*-technique or inverted-factor analysis, analysis of variance and covariance, rating and scaling, and the Fisher discriminant function. In Grant's extensive review (10) he covers experimental design and analysis, psychometric methods, correlation and multivariate analysis, coding and computational techniques, and bibliographies and books for general treatments. The methods include analysis of variance with both inter- and intra-subject variation accounted for, the determination of sample size in experimental design, the interpretation of the extreme deviate or the Studentized u, the effects of variation in criteria on the *F*-test, Yates' contingency methods, non-parametric techniques when the assumptions of normality appear to be unwarranted, scaling methods improving on Guttman's analysis, Grubbs' method for distinguishing measuring instrument inaccuracies from actual product variability, Horst's generalized expression for the reliability coefficient when the number of measures varies from individual to individual, and multivariate tests. To these may be added the *P*-technique to round out the *P-Q-R* triumvirate, interval scaling, homogenous test construction, validation by successive clinical predictions, and matching.

Pattern analysis. Munroe has emphasized that statistical treatment of test data must capitalize on clinical experience and specifically has

urged consideration of combinations of scores in quantifying observations.

> By and large it seems clear that poor results [of quantitative studies of Rorschach data] are obtained when a limited number of the traditional scores (which were never intended for use in isolation) are equated with personality traits without further ado and correlated with external factors. Promising results are obtained when *combinations* of scores are used . . . (**17**, p. 15).

Cronbach (**7**) also has pointed to the inadequacy of conventional treatment of test scores. He acknowledges the simplicity of handling test data by treating one score at a time, but is concerned with the failure of this technique to take into account the interrelations and interactions of scores. Yet there is the problem of describing the many dimensions of personality and considering all the data simultaneously. Cronbach suggests, "If tests could be made more reliable, equally reliable, and equally intercorrelated, clinical testing would more quickly produce significant findings." Here, then, are at least three goals to be pursued.

Recognizing the practical limitations of working with a great number of dimensions of personality, Cronbach (**5**) offers the alternative of reducing this number by the "signs approach" of many investigators, or by the relatively newer multiple regression and discriminant function methods. After discussing the limitations of these statistical treatments in terms of the amount of information lost in the course of the analysis and the involved functions, he proposes pattern tabulation. Even this method is limited in its narrow scope of dealing with only two or three scores at a time. Yet it does permit the examination of three-dimensional data without the need for elaborate statistical procedures and as such is an advance over the handling of but one score at a time—"The technique permits one to summarize the performance of a group of persons on a pattern of several scores, to compare groups and to test differences in pattern for significance."

In a sense, this technique is a reversal of the usual trend of applying group statistics to intra-individual data, for pattern analysis of the latter is well-founded in the study of the individual and now Cronbach proposes an application of the method to group data.

Rabin (**18**), in tracing the history of the Rorschach method in differential diagnosis, also noted the inadequacies of the "signs approach" and concluded that it gave "hints," but not differential diagnosis. However, Zubin (**26**) used pattern analysis over ten years ago and very very recently reaffirmed his belief in its promise as a method useful for psychodiagnostics.

Sells[2] has been developing at the USAF School of Aviation Medicine

[2] Sells, S. B. *Quantitative treatment of projective test data.* 1949 (unpublished).

an approach which "utilizes scoring symbols and devices which have been found useful clinically and . . . fit into simple schemes for quantitative treatment." Applied to psychiatric screening tests, the technique classifies behavior as satisfactory, questionable, or unsatisfactory. For example, in treating group Rorschach data and scoring for neurotic tendency, satisfactory behavior consists of no rejections of cards, 20 or more *R*, 30–40% *A*, no color naming, etc.; questionable behavior includes one rejection, 10–20 *R*, *A*% greater or less than 30–40, one instance of color naming, etc.; and unsatisfactory records are defined as more than one rejection, fewer than 10 *R*, greater than 50 % *A*, more than one instance of color naming, etc. Such scoring is based upon discriminating power of the various indicators (the "signs approach" is seen in Sells' technique) and "empirical examination." The various items may be given relative weights and placed in combinations (patterning) on the basis of (1) discriminating power and, (2) relative importance as defined by frequency of occurrence of the item.

Factorial analysis. Factorial analysis has been widely used in the treatment of various types of test data. Rosenzweig summarized its role as follows: "While not a method of directly appraising or investigating a personality. . . . By factor analysis quantitative data are treated mathematically to uncover the smallest number of basic variables to which the crude observations can be reduced, and to throw light upon the relationships among these variables" (**19**, p. 359).

Eysenck (**8**), using factor analysis in his studies of the dimensions of personality, points out that the method, historically, is an extension of the "method of concomitant variation" underlying all correlation procedures. Four different types of factors can be developed—general, group, specific, and error factors. However, there is little agreement on the meaning of these factors after they have been isolated, and Eysenck lists interpretations which vary according to the investigator:

> . . . Thurstone (1935) and Holzinger (1937), for instance, regard factors as primary or fundamental abilities; similarly, Spearman (1927) regards them as fundamental functions of the mind. If factor analysis is to be applied to the study of personality as well as to the study of abilities, this definition has to be broadened somewhat, and it would become necessary to think of factors as elementary or unitary traits of personality (Kelley, 1935), or as the fundamental dimensions of the mind (Guilford, 1940). On the other hand, Anastasi (1938), Allport (1937), and Thomson (1939) consider that factors are statistical artifacts, having no "reality" of any kind.
>
> Our own position is very similar to that of Burt (1940), who regards factors as principles of classification (**8**, pp. 16–17).

Thurstone, Guilford, R. B. Cattell, and Eysenck, among others, have used factor analysis to extend the amount of information derived from a set of data. We see the traditional correlational techniques built upon to form such analyses as Cattell's "*P*-technique" and Stephenson's

"*Q*-technique" (3, 21, 22). However, the need for more precise definition and for more universal agreement on the *meaning* of factor analysis is again underlined in Stephenson's presentation of his *Q*-technique, just as in Eysenck's comments. Stephenson (21) cites Banks' use of Burt's summation method to refactor a table of correlations which previously had been reduced to eighteen factors by Cattell, and found a different set of factors involving one general, two group, and eight other factors. Stephenson, in turn, refactored the Cattell table by Spearman methods and found four group factors. Further, Stephenson summarizes: (1) Banks considered her factors were structurally related, (2) Cattell's were not, and (3) Stephenson's interpretation of the factors were "very different."

Nevertheless, Cattell has made factor analysis an integral part of his treatment of data in personality study and through such analysis recently has built much of his personality theory. For example, in his exposition of the *P*-technique, the method of factor analysis is the basic structure on which, together with the coefficients which are factored, the technique rests. But the weight of evidence still argues for more universal understanding among those who use factor analysis, if it is to satisfy many of the statistical problems raised by present-day psychodiagnostics. This criticism is emphasized by Loevinger (14) who noted that "A test which is factorially pure in one system [of factor analysis] will not be so according to others." There is very much the need for a common language in factor analysis as in other techniques for treating test data.

P-Q-R-techniques triumvirate. Although the traditional correlational technique, the *R*-technique, is more than a half-century old, only very recently have there been adaptations designed for the specialized problems arising from personality study as we know it today. Thus, there is as recently as 1947, Cattell's article (3) which in its summary begins "*P*-technique, a method employing intra-individual correlation, is tried out for the first time." And Stephenson (21) dates to a decade ago his first application of the *Q*-technique. Cattell in a footnote distinguishes the *P*-, *Q*-, and *R*-techniques thus:

> . . . in *R*-technique we correlated *test variables* with regard to a series of persons; in *Q*-technique we correlate *persons* with regard to a series of tests. In *P*-technique we again correlate test variables, but with regard to a series of occasions and within a single person. Practically all factor analyses yet published have been in terms of *R*-technique and the remainder in *Q*-technique (3, p. 267).

The method of "temporal sequence studies," implicit in the *P*-technique and using intra-individual correlation, has been mentioned by a number of investigators other than Cattell. Nor is the method restricted to correlational techniques, as pointed out by Horn (12) who proposed three methods for treating intra-individual variability in the

study of personality by the questionnaire and personality inventory approach. Horn closes his proposal with an evaluation of the three methods, and concludes that the most promising one for the interpretation of personality inventories is the repeated test technique utilizing ten or more repetitions.

Zubin (27) also suggests repeated administration of diagnostic devices because of the need for modifying or for introducing new statistical techniques in treating the data. He indicates the failure of results expressed in terms of "ordinary group statistics" because the assumptions of randomness and of variability are not fulfilled in the study of abnormal individuals. Because of the requirements of clinical data, he urges that not only is it necessary to sample an individual's performance, but also to sample his variability. To do this, he feels that four or more samples should be drawn in each case.

Again there appears a discussion of analysis of successive measures on the same person in Baldwin's concern (2) with methods for handling the time dimension in intra-individual investigation. The longitudinal studies of the Fels Research Institute necessarily emphasize the significance of the temporal factor in data from a single individual. Baldwin cites as an example the rating of one child for twenty successive days, a number greater than that suggested by either Horn or Zubin. Further, just as Zubin indicated, Baldwin notes that "temporal data are rarely normally distributed." Finally, he recommended "Further mathematical analysis of the properties of time series seems like a profitable investment of effort, and the techniques which such an analysis might produce would permit more meaningful analysis of longitudinal data."

The more traditional *R*-technique also has been adapted to meet other problems in statistical treatment of data obtained by current techniques. Thus, Kogan and Hunt (13) used intercorrelations of judgments in the classic manner to test reliability between groups, but went on to treat the correlations as raw scores in computing *t*-ratios and *P*-values. However, these authors acknowledged that there remained the question whether the *t*-test was appropriate without converting *r*'s to *z*'s. They also treated the standard deviations of the ratings as raw scores, again with "the vexing problem . . . whether or not such standard deviations may logically be treated as raw scores." Finally, analyses of variance in the judges' ratings also were made but still with the haunting question of appropriateness. Because the investigators were dissatisfied with any one of these three methods of testing the null hypothesis, alternative methods were used in the hope that similar results would reinforce conclusions drawn from the tests. Such dissatisfaction with statistics for present-day data has motivated the present survey sponsored by the Committee on Diagnostic Devices.

An interesting variation is suggested by Stephenson (22) who sees the possibility of designing data to fit specified statistical techniques,

whereas it has been almost universal to design statistical methods to meet the problems of certain data. Stephenson proposes that "Trait-universes [which he defines as consisting "Ideally . . . of innumerable 'units of behavior,' or 'observable characteristics,' " after Keynes' discussion of twenty years ago] can also be systematically composed, to suit the needs of variance analysis and factorial design (Fisherian) rather than correlational analysis." Yet, the basic elements of the *Q*-technique are not unlike more familiar statistical methods, according to Stephenson's summary:

> . . . having assessed any one person . . . for a given sample of traits, he may be correlated with other persons who have been similarly assessed, for the same sample of traits. It is the correlation between such assessments, usually for several 'independent' variables at a time, that constitutes the descriptive statistics of *Q*-technique.

Cattell's *P*-technique and Baldwin's "temporal sequence" studies seem to reflect much the same treatment of data, both emphasizing *intra-* rather than *inter*-individual study, along the lines of the trend toward individual-centered statistics observed by Zubin. Cattell (3, p. 267) defines the *P*-technique as "Essentially . . . a method for applying experimental measurement with co-variational analysis to the single case." "Analysis of intra-individual correlations" is seen by Baldwin (2) as the *P*-technique and *R*-technique of Cattell, although the latter (3, p. 269) sees Baldwin's work as "apparently concerned in a different theoretical framework" and distinguishes between *P*-technique and intra-individual correlation as done by Baldwin.

Rating scale techniques. One of the oldest methods of quantifying observations is the rating scale, a device whose use has been attacked again and again, particularly on the grounds of unreliability. Yet the rating scale has survived the tests of use and years and only very recently (28) again has been put forth as the preferred method for putting data in order for statistical treatment. By its nature, the rating scale is primarily a method for quantifying observations prior to statistical manipulation, i.e., a "raw statistical tool" as Zubin (25) defines it.

> . . . scaling devices which try to capture the full clinical flavor of the response and yet put it in a quantitative form. The purpose of these scales is to permit an evaluation of *all* the factors that may have been involved in the formation of the response and not only of those which the Rorschach expert intuitively selects as the most important factors. . . . This is not to say that present-day scoring is lacking in clinical value; but before these clinical values can be integrated into a unified system they will have to be put in terms of scales that are measurable and integrated with other psychometric procedures (26, pp. 18, 19).

Building on this orientation, Zubin and Young (28) developed a series of rating scales classifying response categories into scale steps and

extending the method of scaling to a variety of psychodiagnostic devices. Thus, they have applied the "psychometric approach" in the evaluation of such techniques as the Rorschach, Levy Movement Blots, the TAT, and word association. In preparation there is a volume to include scaling of handwriting analysis, the mosaic test, and the Bender-Gestalt. An example of the scaling technique for "Perseveration Tendency (Pe)" follows:

0. Response totally independent of previous responses.
1. Some slight or general similarity either in form or content to a previous response.
2. Definite similarity in form but not in content.
3. Definite similarity in content but not in form.
4. Exact, or nearly exact, repetition of a previous response.

Such scales, of four or five steps, are defined for Rorschach responses with respect to locale, organization, determinants, interpretation (by the subject of the determinants used by him), content, psychological characteristics of the response (as perseveration, mood, congruence), subject's attitude toward the response, and general data (including number of rejections, inquiry responses, and reaction time). These in turn consist of a series of scales. To define locale, for example, there are scales of importance, type of white space, inclusiveness of primary locale, and frequency of locale. It is obvious from the scope of the scaling by Zubin and Young that such quantification of projective data represents a major effort toward the development of this type of "raw statistical tool."

In a different area of research—social service case work—but with the same emphasis on quantification and reliability of judgment is the work of Kogan and Hunt. These investigators (**13**) also used the rating scale in the primary quantification of data, constructing one by psychological principles and including anchoring case illustrations. Objectivity was thus furthered by having each judge rate by applying the standardized scale to a structured case summary. But Kogan and Hunt report, "In the course of the development and application of this scale a persistent statistical problem has confronted the investigators." Working through the problems in terms of null hypotheses and the broader problem of evaluation, they conclude that their "critical problem is how best to determine whether alternative methods designed to increase agreement among judges really do have such an effect." After using judgments of raters as the raw data in correlations and in *t*- and *F*-tests of significance, final evaluation showed their dissatisfaction with any one of these methods for treating multi-judge reliability.

Here again, the problem seems to lie in the further quantification and statistical treatment of data, with use of the rating scale remaining a satisfactory initial tool for quantification. This further quantification and treatment recently has been developed by Yates (**10**), who now has

methods for treating contingency table problems where the psychologist has data only roughly quantified by rating techniques.

Coding. Similar to the rating scale in that the technique makes data available for statistical treatment is the method of coding. Guetzkow (11) recently has attempted to make explicit some of the thinking on coding. Not only does he present the procedures for transforming qualitative material into unitized and classified data, but he examines the reliability of the coding procedures. After indicating the two operations in coding—separating the qualitative data into units, and establishing "category-sets" for classifying the units—Guetzkow details the frames of reference, dimensionalities, and interrelationships of category-sets. He is careful to recognize that unitizing of data is a problem in coding, in order to avoid the common error of failing to delineate unitizing from the data collection procedure and the subsequent classification process. Different unit levels may be used simultaneously, just as there may be simultaneous use of several category-sets at different levels of generality in order to preserve the richness of the data. It can be seen here how coding resembles the rating scale as a statistical tool for quantifying data, for it will be recalled that Zubin urged scaling as a means "to capture the full clinical flavor of the response" (cf. p. 483, above).

Reliability in coding, Guetzkow specifies, depends upon the accuracy with which the data have been coded, and upon the sample (whether its size and selection are adequate to characterize reliably the behavior studied). The latter is easily recognized as a problem in sampling, and not specific to coding, whereas accuracy of coding is as significant as accuracy in obtaining the raw data. Reliability estimates are developed by Guetzkow in a series of equations for both categorizing and unitizing. Reliability of categorizing involves principally the following: "The proportion of units upon which two coders [the reliability of more than two coders also is evaluated by Guetzkow, so that restriction in number is not necessary] agree may be conceived as the sum of those items which both coders incorrectly classify in the same incorrect way."

The equations for evaluating the reliability of the unitizing of data are based upon comparison of the number of units obtained by the coders. Of further practical importance is the contribution of these reliability estimates to the investigator's decision as to how large an amount of data needs to be check-coded in order to obtain the desired level of accuracy. This contribution calls to mind Zubin's suggestion (27) that "sequential analysis provides the experimenter with a means of determining how many observations are required for testing a given hypothesis."

Interval scales. In quantifying data, rating scales basically define the characteristics of the dimensions scaled. This function of definition has often been the concern of the psychologist. As Gardner (9), Loev-

inger (14), McNemar (15), and others have pointed out, scaling methods have been widely used in studies of attitudes, abilities, aptitudes, and other personality factors. Gardner (9) distinguishes between ordinal scales (so called by Stevens) and interval scales (the units of which are equal throughout the range of the scale), quoting McNemar's argument that ordinal scales give only relative rank order among individuals. Gardner writes that interval scales furnish two kinds of information not otherwise available.

> Although ordinal scales are adequate when we wish to know whether one person is superior to another or simply whether improvement—regardless of amount—has taken place, they are inadequate in two important respects. In the first place they do not permit the determination of the *amount* of growth of an individual in a particular trait. Nor do they, in the second place, permit the comparison of differences in performance of individuals in a particular trait. To provide these two kinds of information, interval scales . . . are needed (9, p. 39).

Further, Gardner proposes a method for developing an ordinal scale in which the assumption of normal distribution is not necessary. It will be recalled (cf. pp. 477 ff.) that this assumption is one which the clinical psychologist often feels is not warranted. Gardner also is of the opinion that it is often more reasonable to expect a skewed distribution than a normal distribution. In transforming an ordinal scale of raw scores from two school grades into an interval scale he fits to the data "the more general Pearson Type III Curve," for which he gives the equation. The method has been applied to achievement scores on several parts of the Stanford Achievement Tests taken by approximately fifty thousand children in Grades 2–9. Gardner sees his interval scale method as useful in the measurement of growth and in the comparison of differences where a skewed distribution seems more likely than the usual assumption of normal distribution.

Homogeneous tests. Setting up as her goal the measurement of one psychological function at a time, Loevinger (14) has developed the technique of "homogeneous tests."[3] She distinguishes between *cumulative homogeneous tests* and *differential homogeneous tests.* In the former, "when the items are ordered according to decreasing number of pluses, each person scores plus up to a characteristic item and minus on all subsequent items. For *differential homogeneous tests*, there is an order of the items such that each individual scores minus up to a characteristic item, plus up to another characteristic item, and minus on subsequent items" (14, p. 527). Loevinger gives in example a measure of emotional maturity. "Laces own shoe," "goes downtown alone," "uses family

[3] It is interesting to note that this same goal has been explored very recently by another method. The Levy Movement Blots (28) have been designed "to reduce the components of the Rorschach stimulus."

car" are suggested by her as items of the cumulative test variety; and preferred types of books, radio programs, and games would represent differential test items.

The motivation for developing the technique of homogeneous tests seems best represented in Loevinger's criticism of psychological tests of abilities, attitudes, and personality traits. She summarized her conception of the present state of affairs as follows:

> In contemporary psychometric practice, it is the rule rather than the exception that two people having the same score on a test will have scored plus on different items. When each score stands for a variety of patterns of pluses, it is difficult to conceive of these scores as measuring anything. Such scores are crude empirical devices known to have some predictive efficiency, but they cannot be called measurements in any strict sense (**14**, p. 507).

Successive clinical predictions. Rosenzweig (**20**) has adapted from the successive approximations approach a method which he terms "successive clinical predictions." Emphasizing the lack of quantitative definition of external criteria of validity of psychodiagnostic devices, he reviews the usual criteria of validity—qualitative and quantitative judgment, correlation with other diagnostic devices, and matching data from two or more sources—and points out the problems accompanying the use of such criteria, including rating errors, lack of "systematic validation of referrent instruments," and the presumptive validity of matching.

The framework of Rosenzweig's proposal is familiar to the research worker. His "method for validating projective techniques projectively" he defines as, "a systematic use of trial and error with a succession of hypotheses being advanced, checked, rejected or revised until finally the character of the instrument has been defined by empirical investigation." The method is implemented by a seven-step procedure combining the judgments of two or more investigators; the administration of the new test and a variety of referrent ones; evaluations from case history, interview, test, and other material sources; predictive attempts, qualitative and quantitative; degree of agreement between predictions and test data; revisions of the hypotheses according to the information gathered in earlier steps; and repetition of the steps with the revised hypotheses. Rosenzweig concludes: "A succession of such experiments in clinical prediction should make it possible for the new device gradually to define its own validity—a procedure of objective self-definition."

Method of the discriminant function. Zubin (**27**) sees promise in the method of the discriminant function as a statistical tool. He defines the technique as one "which serves to integrate a group of variables into a total score such that the maximum differentiation from a contrasted group is effected." Moreover, a very recent development of partial discriminant functions, related to zero order discriminant function as partial correlations are to zero order correlations, may permit

the comparison of two groups postoperatively on a test on which they had not been measured and equated preoperatively.

Examining a problem the converse of the Fisher discriminant function, Zubin (27) also reports a need of a method for discovering psychological types by "fractionating a given population into such sub-groups that the difference between the means on these sub-groups would be a maximum." Stephenson feels that his approach, the *Q*-technique, may be used for determining psychological types by correlating each subject with all the others and factor analyzing the resultant matrix (cf. p. 483, above).

Matching. Since Vernon's application of the blind-matching technique to personality research in the 1930's, this method of treating qualitative data has been widely used (4, p. 367), particularly for interpretations of projective test data. "Blind analyses" are blind in the sense that the personality interpretation is made without actually seeing the patient whose personality is under study.

Just as Zubin (26, 28) has reaffirmed his faith in rating scales for handling qualitative data, so has Cronbach (4) experimented again with the blind-matching technique. Rosenzweig (19, p. 357 ff.) has attributed the popularity of this method as partially a function of the emphasis on the "total individual." He sees as the basic motivation for the development of the matching technique "a comparison of individuals and at the same time . . . regard for the interdependence of the personality in its various aspects . . . as opposed to the rating scale method—no attempt is ordinarily made to break down the personality into segments or parts regarded as more or less independently measurable."

Cronbach (4) objectively reviews the arguments *pro* and *con* blind-matching, weighs the evidence, and concludes from the application of the method to a small study that "It yielded the types of objective analysis necessary for a thorough validation of a qualitative assessment of personality." The procedure for handling three cases at a time in matching is outlined in five steps and then detailed by Cronbach. He concisely presents the advantages and disadvantages of the method.

> The advantages of the method may be summarized as follows:
>
> 1. Neither the bias of the investigator nor of the judges influences the estimate of validity.
>
> 2. The estimate of validity, although using a qualitative criterion, yields a test of statistical significance and a coefficient like conventional validity coefficients.
>
> 3. Qualitative criteria are more meaningful and more truthful than ratings or other quantitative criteria.
>
> 4. The method identifies objectively the accurate and inaccurate aspects of the prediction.
>
> 5. The method permits one to identify the types of cases for whom prediction is relatively accurate or inaccurate.
>
> 6. The disadvantage of the method is that the design is laborious, and the

services of a large number of judges must be enlisted. Since such a large group of judges cannot be handpicked, there is some danger of careless judging which will reduce validity. The difficulty of obtaining adequate qualitative criteria must be recognized, but this problem is not specific to this validation design. The present method is not ideal, since it does not treat the prediction as a whole; but blind-matching, which does treat the prediction as a whole, gives little or no information about the validity of the parts (**4**, p. 373).

Finally, Grant (**10**) has reviewed a special technique which he sees as promising in the use of the matching method. He describes Levene's derivation of a sampling distribution of matches which may be applied in the test of the diagnostic qualities of a complex test, by matching the protocols or scores with the diagnostic group for a number of patients where the judge is told the size of each diagnostic group.

Experimental Design

The previous section on statistical techniques has repeatedly introduced experimental design as a major factor in the development and/or application of techniques. Statistics and design are complements in research, as Grant (**10**) has shown in his review on "Statistical Theory and Research Design." The emphasis in the past pages has been on the statistical techniques, however, and Grant's emphasis is on the second of the two components—research design.

Of interest to the clinician, in addition to the "Proposals" just presented, are certain procedures selected from Grant's survey. For example, the work of Harris, Horwitz, and Mood provides methods that are useful in the problem of size of sample in research. In another area, Nair developed a technique for evaluating an extreme deviate mean, the Studentized u, one found in analysis of variance to differ extremely from a group of means. Grant points out the difficulty frequently found in the interpretation of such a deviate, and sees in Nair's treatment "an objective test for rejection of outlying observations or the evaluation of an unusual mean." Also valuable in analysis of variance is Aspin's demonstration that in a two-sample experiment, absence of homogeneity of variance need not be serious if the two samples are of equal size.

Although there is additional freedom provided by the above techniques, there also is the caution by Grant—of particular note to the clinician who proposes such research—that the plan must include precise control of the independent variable in analysis of variance designs. Madow's example of the sensitivity of the F-test to lack of control of the independent variable is elaborated in Grant's suggestion: " . . . if three differentially diagnosed groups of patients were selected for evalu-

ation of improvement after four different therapies, slight variation in diagnostic criteria or in details of therapy could greatly reduce the power of an F-test used in analysis" (10). Also because of the clinician's attention to diagnosis and prediction, where the samples he studies may differ in two or more variables the problem of multivariate analysis must be faced. Analysis of variance (10) provides tests of significance of differences between groups when one variable is involved, but does not yield tests for differences "in two or more variables, and a test is sought for detecting systematic differences between groups, making use of all available scores." Grant's review of Rao's, and of Rao and Slater's multivariate analysis indicates the value of the method for inter-group analyses.

Summary

Psychodiagnostic devices in clinical practice have contributed significantly to the study of human behavior. Concerned with techniques for establishing the reliability and validity of the diagnostic tools, psychologists recently have faced problems in treating data which have shifted the thinking of the statistically-minded. The projective techniques especially have presented problems.

Traditionally, the assumption of normal distribution has furnished the basic hypothesis for statistical treatment of test data. Empirical evidence has been evaluated by a number of investigators (2, 13, 14, 17) who have concluded that the assumption of normal distribution often is unwarranted. Although the history of statistics in psychology has emphasized the treatment of group data, the emphasis has now shifted to techniques designed to treat data from a single individual in an "intra-individual" (2) or "individual-centered" (27) approach.

Another direction of thinking, and more basic, is one which concerns the *meaning* of statistics for test data, particularly from projective methods. There are those who feel that the underlying assumptions of statistics in psychometrics do not apply to projective test data. Other investigators, however, feel that the assumptions apply equally well to all test data, and that the major problem is the devising of special techniques to fit the data of projective devices.

There is this lack of agreement on the problems of handling test data statistically, yet there has been progress. This progress is seen in a number of statistical techniques proposed—some dealing with the quantification of data so that they can be ordered for statistical treatment, e.g., rating scales and coding; others concern methods for treating data already ordered in codes, scales, correlations, or other distributions. These include the more familiar correlational or *R*-technique, and

factor analysis of the correlational data. Techniques proposed more recently include the *P*- and *Q*-techniques, pattern analyses, discriminant and partial discriminant functions, interval scaling, homogeneous test construction, successive clinical predictions, matching, analysis of variance and multivariate analysis.

Despite the lack of agreement among the various investigators, it is significant in the current emphases of the clinical psychologist that he is more and more concerned with rigorous examination of his techniques and of his methods for treating the data furnished by the diagnostic devices. Within the past two years psychological meetings have featured symposia titled "The problems of quantification and objectification in personality" (**25**) and "Statistics for the clinician" (**29**), as witness of the importance of these problems to the student of personality. Such interest and effort are encouraging research toward the goals of reliability and validity in the study of personality.

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RORSCHACH VALIDATION: SOME METHODOLOGICAL ASPECTS

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INTRODUCTION

It is generally agreed in clinical circles that the Rorschach psychodiagnostic method is the most comprehensive device in the clinician's repertoire for ascertaining personality patterns. However, it is also agreed that results are highly dependent upon the skill of the interpreter. This is due to the fact that the relationships between the data yielded by the test and personality variables have not been clearly ascertained and stated. Consequently, the user of the method must rely upon a body of guesses as to the relationships involved—including those which have accumulated in the literature, plus his own experience with the test in particular, and knowledge of normal and abnormal personality in general.

Considerable space in psychological literature was at one time devoted to the question of whether or not such a state of affairs was desirable. More recently the majority of those concerned with use of the instrument have supported the position that the Rorschach needs validation (**1**, **2**, **6**, **8**, **15**, **16**, **17**, **21**, **26**, **30**, **32**, **35**, **36**, **39**). However, the variety of opinions on the requirements for Rorschach validation are as numerous as are the theoretical and methodological positions in the entire field of psychology—as the following quotations will demonstrate.

Harrison (**15**) claims that rigid validation procedures may be applicable to cognitive aspects of the personality but not the affective aspects. He maintains that "there must be sacrifice of objectivity and spurious precision with more reliance being put upon insight and ingenuity in the forging of tools for personality investigation." Clinical validation, he claims, is ample evidence for the present, though controlled experimentation may in the future produce more definite knowledge. Rapaport (**26**) and Brosin and Fromm (**6**) call for experimental investigation of "what form level means" by use of Gestalt experiments of form genesis and prägnance. Brosin and Fromm state further that " . . . the question of what may constitute the basis for correspondence between the responses and the personality of the subject; and the nature of the equivalence between the perceptual patterns and the general personality patterns, seem to obtain the most fruitful suggestions from Gestalt Psychology."

Sargent (32) claims that "factors taken out of context have little meaning. For the reason that it is not the absolute amount of one determinant but its relation to the whole pattern which gives it significance in the individual protocol." Thus, for her, validity studies can proceed along two lines: (1) correspondence of Rorschach impression of the total personality with impressions independently arrived at, and (2) predictive capacity which she thinks is the sounder approach due to the fact that correlations based upon inter-judge agreement are minimized by "each analyst understanding correctly different aspects of the personalities under inspection."

Macfarlane (21) lists five validation methods: (1) correlation with outside criteria, (2) projective data vs. case history data, (3) through time consistencies, (4) collateral experimental approach, and (5) degree of success in prediction, which she offers as the most fruitful method. In support of this method she says:

> The writer's opinion is that the utilization of the interpretation and predictive judgments of widely experienced clinicians later checked for predictive success, will offer at this stage the most productive leads. If an experienced clinician is able to predict with considerable success, then the data on which he bases his correct intuitive predictions can be inspected and validly weighted configurations can be established, quantified, and made available to less experienced people. Also, his wrong and partially right predictions in conjunction with the right ones can serve for finer and more differentiating criteria.

Frank (8) holds that validation must be concerned with the aggregate of part functions since he believes that there may be lawfulness at that level but not between the discrete part functions and outside criteria. Like Harrison, he appears to believe in the acceptability of intermediate gross validation, for he says:

> If it appears that the subject projects similar patterns or configurations upon widely different materials and reveals in his life history the sequence of experiences that make these projections psychologically meaningful for his personality, then the procedures may be judged sufficiently valid to warrant further experimentation and refinement.

Thurstone (35), in a recent polemic against the scientific laxity of Rorschach workers, calls for validation data that will enable a Rorschach interpreter to predict the style of response an individual is likely to give to different types of life situations. Hertz (16, 17) has been quite outspoken about the need for validating data. She suggests four methods: (1) direct experimentation, (2) comparison with extensive individual case studies, (3) comparison with independent, objective data, and (4) comparison with known diagnoses and clinical pictures. In her earlier

article (16), she states that concentration on one individual in the tradition of the clinician would be scientifically productive.

Varvel (36) has offered some specific suggestions for validation procedures. He mentions the possibility of finding the relationship between the autokinetic phenomenon and Rorschach personality types, checking Rorschach factors against data obtained from the Dembo situation, use of drugs and post-hypnotic suggestion to measure the effects on Rorschach protocols of these experimentally produced states, and simultaneous recording with the Luria technique, pneumograph and Rorschach responses.

In 1935 Beck (1) wrote that the value of the Rorschach method was not on the basis of successful penetration into personality as yet, but as a suggestion that human nature could be passed through a prism and analyzed into component elements. He cautioned, however, that it must be recognized, " . . . that the same trait is not necessarily equivalent in two different kinds of personalities, or in another way, the same diagnostic index may have different diagnostic value depending on the larger background of the personality in which it appears." In 1942 (2) he wrote that Klopfer's argument that standardization of the Rorschach implied devising of numerical formulae for the several whole personality entities, was a fallacy. He pointed out that two issues were confused in Klopfer's position. One was the personality as a whole governed by the laws which unit personalities follow and the second was the component elements, " . . . whether we are contemplating the psychological traits or the impersonal Rorschach factors which stand for these traits." Of the latter, Beck claimed that they can and must be isolated and subjected to experimentally controlled observation independently of the problems relating to the whole personality. "The criteria of the two are different and validation is within two totally different spheres of reference."

Point of View

As is obvious from this sampling, a position on the most fruitful approach to take in future Rorschach validation work is dependent upon one's theoretical and methodological biases in the broader problems within the fields of science and psychology. Accordingly, before embarking on a set of criticisms and suggestions, it is necessary in the interest of clarity to make an explicit statement of position on the crucial issues involved.

Validity is here considered to be measured by a statement of the degree of concomitant variation between two independent variables,

one of which is designated as the criterion variable. The data obtained from a validity investigation indicate the degree of predictability of the magnitude of one variable, given the magnitude of the other. In the problem of Rorschach validation, then, the usefulness of the results will depend upon the criterion chosen. The requisites for an acceptable criterion variable are: (1) amenability to the assignment of numerals with a high degree of reliability which represents a relative statement of the degree of presence of the variable, and (2) known or hypothesized relationships between the criterion variable and other variables useful in work with personality. Use of the term "numerals" in (1) above indicates another bias. The writer believes that the variables of personality are lawfully related to relevant environmental variables and since laws are numerical statements for optimal serviceability, it is necessary to quantify the variables in order to find the laws. This is deemed a requirement for a science of psychology. Finding the laws depends in part upon a methodological approach geared to finding lawful relationships. It then becomes an empirical matter as to whether or not the lawfulness is "out there."

In the case of the Rorschach it is held as a working hypothesis that many of the variables of personality can be measured by it, the task being to ascertain the tenability of the hypothesis by making a series of verifiable hypotheses relating clearly defined personality variables to each of the factors or groups of factors found in a Rorschach protocol. It is also held that the laws describing the functioning of the whole personality are of the same class as those relating component parts to environmental variables. In describing the whole personality, what is needed is a composite law expressing the component parts and their interaction. Again, this may be true or false according to future empirical findings. Its immediate value lies in focusing on a search for relationships that can be expressed in a single system of laws. As a practical matter, it is probably more fruitful to start with component parts until more of the relevant variables for the total personality are ascertained. The rest of this paper will be devoted to an application of this orientation in an attempt to clarify some of the methodological problems inherent in Rorschach validation.

Levels of Inference

From a similar theoretical and methodological background, Steisel and Cohen (**34**) have elaborated on Beck's differentiation of the levels of inference used in interpreting from Rorschach protocols. To clarify the problems of Rorschach validation they differentiate three levels of inference, each of which require validation.

Level I covers the inferences drawn from the raw response record for purposes of arriving at the appropriate scoring symbols and constructing an adequate psychogram.

Level II represents the descriptive statements which can be made by inspection of the psychogram, sequence analysis and analysis of specific verbalizations.

Level III includes the comprehensive, coherent personality diagnoses which may include the classification of the subject in some nosological group.

This can be considered a vertical schema ascending in order of degree of complexity (the number of variables involved in attempts to validate the inferences made). However, it can be pointed out that Level I does not involve the problem of validity as herein defined. Steisel and Cohen's Level I can more adequately be defined as entailing the problem of reliability, that is, the setting up of symbols each with a definition which summarizes certain aspects of a given response. The problem is to find the definition which will yield the highest degree of agreement in the case where several observers using the same definition for a given symbol are tested for the degree of concurrence of their applications of the symbol in question.

Having established that certain symbols can be applied with a satisfactory degree of reliability, Level II is reached. Level II represents the inferences as to specific processes or mechanisms which can be made about a personality. The raw data for these are found mostly in the psychograms (factors such as *F*+%, *M*, *C*, *W*:*M*, etc.) though material such as symbolism, pathognomic signs, and sequence analysis (19) may also be used. As a point of departure some of the Rorschach studies sometimes referred to as validation studies in the literature will be classified in terms of the Rorschach variables used and the method employed. Each study will be evaluated in terms of the notion of validity expressed above.

Level II: Psychological Processes Represented by Psychogram Factors

1. *Hypnosis.* Sarbin (31) took Rorschach protocols from a single subject under four different conditions. Two of these conditions were with the hypnotic suggestion that the patient was Mme. Curie, and then Mae West; a third condition was that of hypnosis with no suggestion; and the fourth condition was in the waking state. He found that each of the three artificial situations produced changes in several of the psychogram factors and he attributed the changes to induced sets. However, this does not qualify as a validity study in that the relationship between the particular stimuli used to induce the sets, and changes resulting, are not known.

Levine, Grassi, and Gerson (20) used hypnosis in a Rorschach study

with an improved method. They obtained a Rorschach protocol under normal conditions and then under several conditions of hypnotically induced affective changes. They suggested certain situations (under hypnosis) the psychological consequences of which were predicted on the basis of clinical experience. The Rorschach protocols obtained under the experimental conditions reflected changes in psychogram factors for each of the induced conditions. Further, the changes obtained in each case reflected the Rorschach factors usually associated with the respective affective conditions induced. Validity of the results in this case, is, of course, dependent upon the accuracy with which a group of judges can predict the psychological correlates of certain situations. Accurate prediction in such cases is dependent in part upon knowledge of the differential reactions of a large group of subjects to the same situation. Thus the relationship between the factors of the emotionally charged situation and the personality involved must be known in order to predict for small groups. Also, since it is known that hypnotizability is related to certain aspects of personality, results of such studies cannot legitimately be generalized beyond groups that can be hypnotized.

Bergmann, Graham, and Leavitt (4) used Rorschach protocols in conjunction with hypnotic age level regressions. The authors do not claim this to be a Rorschach validity study. They acknowledge using Rorschach personality pictures obtained at the various suggested age levels to validate the extent of regression, in that superficial regression would not be expected to reflect a complete change in Rorschach patterns. However, were the extent of regression under hypnosis established by use of independent measures other than Rorschach, then Rorschach factors could be validated by this procedure. Detailed histories of certain levels of individual development could be obtained and a group selected on the basis of optimum uniformity. On the basis of the personality features held in common for the individuals of this group, predictions could be made as to the relevant Rorschach factors that would be obtained under hypnotic regression to the respective levels of similarity. However, such a roundabout method is warranted only if there is a demonstrable need for an approach to the study of personality such as that used by Bergmann, Graham, and Leavitt.

In general, it might be said of use of hypnosis for Rorschach validation that considerably more data are needed to demonstrate how much of the personality changes are produced by the nature of the situation (i.e., effect of submission of the passive individual to the assertive hypnotist) and how much by the specific suggestions used.

2. *Drugs.* The studies reported in the literature (13, 18, 27, 37, 38) relating Rorschach factors to administration of drugs do not qualify as validation studies in that the laws describing the relationship between the drugs used (mescaline, histamine, amytal) and personality factors measured independently of the Rorschach are not known. The data

reported suggest that the reactions obtained depend in part upon features of the predrug personality. Guttman (13) states, " . . . there is need of a searching investigation in terms of the subject's setting and individual equipment before specificity of reaction can be established." Until the laws relating a single drug with personality reactions are found, this method will not be serviceable in Rorschach validation work.

3. *Operationally Defined Situations.* As Williams points out (39) this method requires: (1) setting up of an "independent and standardized criterion situation which would yield a quantitative index to an operationally defined aspect of personality, and (2) examination of the validity of those Rorschach factors primarily associated with this personality process in terms of the criterion measure." He assembled the definitions offered for intellectual control and designed a situation to measure this. He used as his measure decrement in performance of a task requiring intellectual factors under stress compared with performance under optimum conditions. Thus, the definition of intellectual control is in terms of the operations used to measure it. He administered a standard Rorschach to each of his subjects and used as the Rorschach index of intellectual control $F+\%$ and integration of form with color on the basis of hypotheses stated by Beck and Rapaport. He acknowledged the fact that other factors also were used in Rorschach interpretation and that he was operating at what is considered in this paper to be Level II. His results tend to bear out the hypothesized relationships between the selected Rorschach factors and the operationally defined aspects of personality. Structurally, this study conforms with the tenets presented here. However, he overlooked the fact that a linear relationship between intellectual control and $F+\%$ is not hypothesized by Beck and Rapaport for emotionally charged situations. An $F+\%$ above the optimum hypothetically represents an overcontrol due to inhibition and anxiety, compulsiveness, and rigidity or strong superego. Consequently, it might be hypothesized: (1) that overly-acute form perception is a defense against affective instability and as such might be related to loss of control under affective stress, and (2) that the threshold for affective stress would be lower than that for an individual with a lower but optimal $F+\%$. Therefore, it might be more in line with expectancy to adujst for $F+\%$ above the empirical mean for a standardization population, rather than consider it to be a continuous distribution with respect to intellectual control under stress.

Another study with a similar methodological basis provides a focus for the problem of the usefulness of the independent variable with which the Rorschach factors are correlated. Steisel (33) attempted to demonstrate a relationship between the factors on the Rorschach commonly hypothesized to be related to suggestibility, and performance in the autokinetic and Hull body sway situations. The relationship between

suggestibility and performance in these situations is not as universally acceptable as is the relationship between intellectual control and the situation used by Williams. Therefore, acceptance of Steisel's results as bearing upon suggestibility as a personality variable depends upon the demonstration of a relationship between his two criterion measures and another independent measure of suggestibility. Insofar as his concept of suggestibility is operationally defined in terms of these two indices it is a defensible method at the current stage of development of Rorschach validation; i.e., if he had found a relationship between his selected variables it would then be a fruitful problem to experiment with the autokinetic phenomenon and Hull body sway technique to find their relationship to other independently defined aspects of personality. Even if they should be found related to aspects other than suggestibility, Steisel's data would be useful.

Rockwell, Welch, Kubis, and Fisichelli (**28**) report an experiment concerned with color shock. They define shock in terms of the neurological conception as a condition of lowered excitability. They measured shock by taking a continuous reading of the palmar skin response during Rorschach administration and described the changes expected with "shock" present and those expected with startle. Thus, their measure constitutes an operational criterion for shock and startle. They found that shock, as neurophysiologically conceived, does occur on those cards and in those subjects where it is expected, whereas startle does not. On the basis of these results they reject the Beck criteria for "color shock" which are based on expectation of a startle reaction to the color cards. This is a rather clear-cut example of an operationally defined situation devised to test two antithetical hypotheses. Acceptance of their interpretation depends upon evidence for relationship between the neurological conception of shock and Rorschach's conception of emotional inhibition. Assuming this relationship, it then remains to be determined what conditions the startle response (or more properly the Rorschach criteria for its presence) is related to—as a problem for those interpreters who use the "startle" criteria.

4. *Empirical Sign Studies.* The studies that fall into this group are concerned with demonstration of a relationship between certain single psychogram factors or groups of factors and effectiveness of various therapeutic techniques. (The sign studies concerned with relationship of Rorschach factors to psychiatric diagnostic categories will be dealt with in the series of Level III studies.)

In 1940 Piotrowski (**23**) published data which resulted from a study of two groups of schizophrenics, one of which showed improvement following insulin therapy while the other did not. He compared the pretreatment Rorschach protocols, and found that the presence of a response which included color as a determinant (*FC*, *CF*, or *C*) and for which there is a concrete or emotional association, i.e., other than color denomination, occurred significantly more frequently in the records of

those who subsequently improved than those who did not. He reported that 81 per cent of the improved group met this criterion. Halpern (**14**) in a study of similar design found that the pretreatment Rorschach psychograms of those patients who subsequently improved with insulin therapy manifested greater productivity and those factors associated with "wider emotional range and greater capacity for empathy." Graham (**9**) with a study similarly designed found that those who improve with insulin give more responses determined by the chiaroscuro aspects of the inkblots. Benjamin (**3**) gives a preliminary report of a follow-up study of insulin-treated schizophrenics for whom he had pretreatment Rorschach protocols. Though his data are unquantified, he states that the preliminary suggestion is that the appearance of low *F+%*, *DdW* and *DW*, and highly irregular sequence are associated with poor prognosis.

In these studies the criterion variable is judged improvement, and consequently judgment of improvement must be based upon clear-cut reliable criteria. Also, a clear statistical statement of the relationship between degrees of improvement and the signs reported to discriminate between "improved" and "unimproved" is required for an acceptable statement of validity. The above studies do not meet these criteria, but the results suggest possible relationships to be checked by more rigorous methods.

In an investigation concerned with factors related to success of metrazol therapy with a group of psychotics, Morris (**22**) improved on the method in the preceding studies. He provided for more clear-cut criteria for judgment of improvement and then selected critical points for his signs by referring his findings to the chi-square distribution for a statistical statement of his results.

The significance of this group of studies lies in the attempt to establish the usefulness of the Rorschach for prediction under restricted, specified conditions. However, they contribute little to validation of the Rorschach in terms of the second validity criterion, which calls for correlation between Rorschach variables and independent measures of personality for which a relationship with other personality measures is known. Should it later be demonstrated that improvement following insulin or metrazol administration is related to certain dimensions of personality, then the signs found in the above studies can be used as a basis for statements about these new dimensions insofar as the studies from which they result are experimentally sound. It should be pointed out that though these studies may not be of immediate optimal value for Rorschach validation, this does not mean they are equally questionable from the standpoint of other theoretical and/or practical considerations, such as the ones for which they were designed. They are on the same continuum only with respect to the first criterion of validity and are cited here merely to clarify the issues involved in Rorschach validation per se.

Level II: Psychological Processes Represented by Factors Other Than Those in the Psychogram

1. *Symbolism.* Earl (7) reported an experiment in which he investigated the validity of the usual interpretations for certain content categories frequently obtained in the Rorschach protocols of disturbed subjects. He was primarily interested in water responses and selected a group of feebleminded males aged thirteen to fifteen who were inmates of a residential school and had markedly immature personalities with severe conflict over masturbation. He selected those words with water content and all content words which occurred with the determinants *M*, *FM*, *m*, *K*, *FK*, and *KF* in the preexperiment protocols. He then put his subjects in an hypnotic trance and told them, "I am going to tell you to think of something. As soon as you think of it something quite different will come into your mind . . . I want you to say at once what it is. . . ." He then gave as stimulus words the words taken from the subject's own record which were suspected of being symbolic, interspersed among banal responses from his own record. He found that all the crucial words used for expressing a response the determinant of which was *m*, had symbolic significance. All responses having water as primary or secondary content "produced anxiety symbolized or overt in the hypnotized subjects." In interpreting his study he suggests that no common symbolic significance exists, at least among children, and that these responses, however determined, are associated with profound anxiety and it may be suspected that primitive sexual stirrings are always present.

The nature of his sample and small *N* (5 subjects) places serious restrictions on interpretations of these data. It would be desirable to have a control group of "stable" individuals so as to measure relative occurrence of water responses, and to compare the associations offered in the experimental situations by those "normal" cases in which water responses do occur with the responses of the experimental group. It might also be possible to do this type of work by use of word association without hypnosis, thereby freeing it from the limitations of hypnotic validation studies discussed above. By use of such a method, it is reasonable to expect that affective disturbances associated with the responses suspected of symbolic usage could be demonstrated. However, it is a more difficult problem to ascertain consistency for the particular material being symbolized by a given symbol. Demonstration of such consistency may require an approach which integrates data for responses with data on behavioral indices which are associated with aspects of the inkblots. An example of an attempt to associate a behavioral index with features of the blot is Blake's empirical study of ocular activity (5) during administration of the Rorschach technique. He found that number and duration of fixations was related to properties of the cards.

Card VI, for example, elicits the largest number of fixations for the top center detail. Card VI as a whole also elicits longer fixations during the initial inspection period than is true of the other cards in the sequence. It is also known that the top center detail of Card VI elicits the largest number of manifest sex responses and is suspected of producing symbolized sex responses as well as disturbances of sequence, form, control, etc., which are referred to as "sex shock." Therefore, it might prove fruitful to select those areas of the several cards which yield the highest number of fixations and use the responses offered for them in a method similar to that used by Earl. One might use a large group of normals and investigate the number of times a particular word is substituted in the word association experiment for the response offered during Rorschach administration.

2. *Empirical Sign Studies.* In 1941 Pitrowski (**24**) published a paper which presented six signs relevant to predicting the prognosis of schizophrenics given insulin therapy. It is unlike the similar studies reported above in that five of his signs are not found in the usual psychogram. He presented six signs which are reported to be indicators of a good prognosis: Generic Term, Variety, Evidence, Color Response, Indirect Color Approach, and Demurring. He offers what seems to be adequate criteria for determining presence of responses with these attributes, designates the method of determining improvement (psychiatric staff decision), and claims 93.3 per cent correct prediction by use of three or more signs as the criterion for good prognosis. As in the other studies of this type it remains to be demonstrated that prognosis for schizophrenics with insulin therapy is lawfully related to certain specified personality variables or processes. On the basis of his own data, Piotrowski offers an hypothesis in terms of intellectual regression to account for the differences between schizophrenics who improve with insulin and those who do not. Those who improve have undergone emotional regression; those who do not improve have undergone intellectual regression during their pretreatment illness. Assuming that there are operations with which to measure intellectual regression (other than Rorschach variables), this hypothesis then becomes subject to experimental verification. If verified, Piotrowski's signs would then become valid indicators of emotional regression (though the converse could not be assumed a priori in the case of absence of the signs). This is an illustration of how a sign study which, though not of itself a complete validation process, might lead to a series of studies the end result of which is validation of Rorschach variables. However, before results such as Piotrowski's are followed up, it would be advisable to select a new group of schizophrenics about to be treated with insulin shock and test the accuracy of the prediction of his signs. This would guard somewhat against the possibility that his original results were influenced by variables other than those to which he attributed his results.

Level III: Comprehensive Personality Diagnoses

At this level an attempt is made to relate variables of the Rorschach protocol to groups with various diagnostic labels. The emphasis is on a complete personality pattern rather than the component processes as far as the validity criterion is concerned. Thus one might measure the degree of relationship between diagnosis arrived at by use of the Rorschach and diagnoses made by any other means such as other clinical tests, psychiatric observation, a combination of tests, interviews and observations, or case history. Several such studies have been reported in the literature with results supposedly supporting the validity of the Rorschach as a diagnostic tool. However, the instability of current diagnostic groupings makes them unsuitable for use as validity criteria. Diagnostic practices differ from institution to institution and even from psychiatrist to psychiatrist in the same institution. A further criticism of this type of study can be made in terms of efficiency which would hold even if stability of diagnostic groupings were granted. The number of variables involved in arriving at a Rorschach diagnosis is large. Some of them might be highly related to the criterion and some not at all related. If the relationship of each variable (used in the Rorschach diagnosis) to the criterion variable is not known, then no statements can be made on possible improvements of the method of diagnosis. It is an all or none conclusion where there is ample reason for suspecting that some of the variables in combination might be more effectively used than others. Guirdham (**10, 11, 12**) has indicated one way to handle this problem in his work with epileptics and depressives in which he sought complexes of Rorschach variables each of which had some power to differentiate the groups in question from other nosological groups. He empirically tried numerical relationships between these variables until he found an index which provided optimum differentiation. The logical development of such an approach would lead to statistical analysis of a large number of Rorschach records from a variety of diagnostic groups, with each Rorschach variable being put into a separate multiple regression equation for each diagnostic category. Such an approach would constitute a sign method and, although a high order one, would still be subject to the limitations of sign approach as a validity method discussed earlier in this paper (i.e., the variables used for prediction are not selected on the basis of degree to which they enter into relationships with other variables of personality).

Piotrowski (**25**) has outlined three approaches to Rorschach diagnosis of mild forms of schizophrenia which can be applied to the general problems of Level III validation. He discusses first pathognomonic signs such as contamination, fluctuations in form level, and position responses. The limitations of this approach he lists as use of too small a part of the total record, and the observed fact that a large proportion of schizophrenics produce none of the signs, and thus absence of the signs does

not mean schizophrenia is not present. He takes up next tabular diagnostic procedures which are based on Rorschach components that can be counted. It consists in establishing the differences in the frequency with which the scoring symbols occur in the records of schizophrenic and nonschizophrenic subjects. He indicates his preference for what he calls a systematic diagnostic procedure which "rests on principles which interrelate the components of the Rorschach method, expressing their mutual dependencies, and arranging them into a dynamic system." By this means he implies that the interrelated variables he uses are based on valid interrelationships among Rorschach symbols, e.g., that the more human percepts, the larger the percentage of sharply perceived forms.

His criticism of the pathognomonic and tabular procedures are well taken as far as he goes, but he overlooks the fact that the interrelationships used for the systematic procedure are not validated in the strict sense. Their "validation" rests on unquantified clinical observations. Further, the search for such relationships as used by Piotrowski requires a deduction as to component processes from what is known about the unit personality of schizophrenics. Such a search would be warranted only if the component processes for a given diagnosis were known. With regularly occurring relationships between given diagnostic categories and component processes we would have stable diagnostic categories, provided there were also operations to measure them. If these operations were independent of the Rorschach then there would be validity criteria for the Rorschach variables. However, in such an ideal situation Rorschach validation would then conform with the description for Level II validation.

Conclusions

In view of the foregoing considerations, the position is taken here that Rorschach validating procedures can be most fruitfully treated as problems in relating Rorschach variables to independent measures of component personality processes. There is no body of facts that can be invoked to prove this contention. It rests on principles of methodology concerned with the demonstration of valid relationships which are acceptable within a scientific framework. The merit of this approach applied to the Rorschach will be substantiated or discredited by the results obtained from its use. If procedures are used with provision for precise statement of results which are repeatedly obtained under the specified conditions, the Rorschach becomes a surer clinical device and it will facilitate psychological research. The clinician frequently protests that the experimental psychologist deals with artificial situations which are in no way related to the behavior which must be dealt with in the clinical problems of diagnosis and treatment. If the relation-

ship between one or more Rorschach variables and component personality factors could be demonstrated, then those factors could be subjected to controlled experimentation with the Rorschach used to measure the changes that occur under varied conditions. It might also be possible to develop relationships between certain manipulable conditions and the origin of some of the personality components. Thus, a single measuring instrument, satisfactorily validated, could help to establish a connection between experimental results and clinical problems. It would seem particularly useful to aim toward this possibility in view of the widely held hypothesis that the Rorschach test samples a larger number of variables of personality than any other single instrument.

In conclusion it should be made explicit that the point of view on validation presented here does not do violence to the experienced interpreter's demands that each Rorschach factor be interpreted in terms of the other factors for any given record. What is required is specific statement of any particular constellation that is believed to be related to some measurable aspect of behavior, and then an appropriate test of this statement.

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CRITIQUE OF HORNELL HART'S "SOCIAL SCIENCE AND THE ATOMIC CRISIS"[1]

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Recognition of the importance of war, peace, and social change as research problems for social scientists has been expressed in the $1000 bond award which Mr. Bernays has asked SPSSI to administer. A distinguished committee of social scientists chose a work of Professor Hornell Hart for the award in 1948, and the summary of his book was published as a supplement to the *Journal of Social Issues*. Both author and editor expressed the hope that the thinking of Prof. Hart would provoke discussion and further planning.

Hart's underlying propositions seem sound and well documented: that over the centuries the power to destroy has increased at an accelerating rate while the power to control such forces—although increasing—lags so far behind that destruction may overcome our civilization.

It is Hart's proposed solution to this problem which strikes me not only as wishful thinking but as dangerously misleading. The fact that his formulation is selected as the best contribution to the problem by so distinguished a committee leads me to believe that my objections to Hart's proposals are not shared by many. I shall formulate my objections, then, as sharply and clearly as I can.

Hart's analysis and program carry, I believe, three false and misleading assumptions which are so fundamental as to vitiate his program, and perhaps lead in directions opposite to those stated.

1. Hart seems to assume that knowledge, reason and equalitarian justice, implemented by persuasion, are the ruling factors in social organization; and that self-interest and the possession of an advantage in power and/or ownership are of negligible importance. He says, for instance, that depressions and war are examples of cultural lag which can be caught up "only [by] a swift upsurge in the effectiveness of social science" (p. 5). "Cultural evolution produces . . . a wide variety of culture complexes which are not adequately integrated. . . . Two outstanding examples of this tendency are economic depression and war" (p. 10). "At present social science is radically inadequate for grappling with the atomic crisis" (p. 14). "The crux of the atomic crisis is the lag of social science behind the accelerating upsurge in the technologies of

[1] HART, HORNELL. Social science and the atomic crisis. *J. soc. Issues*, Supplement Series No. 2., April, 1949. A condensation of the contribution which won the Edward L. Bernays Atomic Energy Award of the Society for the Psychological Study of Social Issues.

destruction" (p. 17). Thus, according to Hart, the ills of the world, including wars and depressions, are due to simple lag; and to bring about world peace and prosperity he proposes his prize-winning project in social science research.

But reality stands to the contrary. What retains wars and depressions is not the absence of knowledge, but institutionalized "vested" interests. Labor-management disputes are settled largely by tests of strength. Slavery was not given up as a consequence of an educational program on race differences; on the contrary, a mythology of race was formulated and promulgated at home and abroad to justify and perpetuate discriminatory practices.

Although Hart's program ignores the power-structure of society, it is obvious that his proposed band of social science experts, working out the program for social change, social control, etc., abandon the whole structure and ideology of the "free enterprise system," and move in the direction of some kind of technocratic, planned economy. Not that I hold any brief for competitive or monopolistic capitalism, but Hart's program must quickly reach the power-arrangements of the status quo, and go with it or against it. Money and resources are more readily available to programs that extend and tighten the established controls than to programs that call for relaxing autocratic controls and spreading power and benefits more widely. Hart's program, by ignoring the central power relationships, tacitly accepts and endorses the status quo —even perhaps bids for its economic support. This suggests the possibility of its being subsidized to become an instrument of resistance to social change.

2. The second and third "wrong assumptions" may be deduced from the following quotations, which are part of a questionnaire Hart submitted to a group of students in a preliminary study concerning "the relative desirability of various possible outcomes of the Atomic Crisis" (p. 22). He asked them "three hypothetical questions":

> 1. If we had to choose between giving Russia a free hand, and engaging in a fairly evenly balanced atomic war with her, which would do the least damage in the long run to the basic values of civilization?
>
> 2. If there were one chance in ten that World War III could be avoided for the next 25 years by working peaceably through the United Nations with all its defects, and four chances in ten that Russia could be rendered helpless by the scientific use of bacteriological and radioactive sanctions, before she could damage America seriously, which course of action would be most likely to preserve the basic values of civilization?
>
> 3. If there were five chances in ten that the United States could maintain law and order in the world by independently expanding her naval and air forces, and by keeping far in advance of other countries in military research, and two chances in ten that destruction could be avoided by creating a cooperative federation of the democracies and pooling all our military resources

together, which policy would be most likely to preserve our basic values? (p. 22).

I wish to emphasize two assumptions from these quotations. First, it is assumed that the people of the United States, their government, and their major dominant policy-making groups, are unified, and that this unity includes agreement on altruistic, world-wide goals, and that peace is high among them. The other assumption, reflected in the phrase "preserve our basic values" and "values of civilization," implies in this context that the American values, as expressed by the current policies of the United States Government, are morally right and are identical with the basic values of civilization. In parallel it assumes that the policies and acts of the political leaders of other states, insofar as they differ from United States policy (especially, that is, Russia) are evil, wrong, and destructive to "civilization's values."

Such a view ignores the important body of facts, readily available for instance, regarding power politics (**1, 5**); world relations toward Soviet Russia (**4, 5**); our own State Department's distortions of historical events regarding the Soviet Union (**6**); the expanding post-war American imperialism (**2**); and the post-war effects of the general policy upon our fundamental democratic rights (**3**).

Hart's questionnaire follows the naive ethnocentric identification of "we-with-God" versus "they-with-the-Devil" for the parties set up in the conflict; and with moral self-righteousness, modestly proposes for serious consideration a war of extermination against the Soviet Union as one way to peace and the expanding values of civilization. I do not see how this implies the use of accelerated social science to overcome the growth and extension of destructive power.

A point of view similar to Hart's is expressed in simple terms of economic and military mastery by Dr. Virgil Jordan, President of the National Industrial Conference Board, in these terms:

> Let us first offer the utmost capacity of our economic power for reconstruction freely to every people who will undertake to abolish all national military expenditures and disarm down to the level of the local constabulary.
>
> Let us, secondly, demand the unlimited right of continuous inspection and control of every industrial operation and process and every public policy which may have the most remote relationship to armament and warfare.
>
> And finally, let us make, improve and keep plenty of our best and biggest atomic bombs for that imperative purpose: let us suspend them in principle over every place in the world where we have any reason to suspect evasion or conspiracy against this purpose; and let us drop them in fact, promptly and without compunction, where ever it is defied (**2**, pp. 175–176).

I hope I am wrong, but I find nothing in the assumptions which underlie Prof. Hart's proposals toward solving the Atomic Crisis that

would disagree with the position of Dr. Jordan, who suggests the military and economic domination of the globe by the group of which he was president.

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REJOINDER TO GUNDLACH'S CRITIQUE

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Dr. Gundlach's first objection appears to be summarized in his statement: "What retains wars and depressions is not the absence of knowledge, but institutionalized 'vested' interests." He fears that a research program which does not "call for relaxing autocratic controls and spreading power and benefits more widely" may be "subsidized to become an instrument of resistance to social change." His position appears to be in line with the familiar dogmas of Socialists and Communists. One of the major tasks of social science (as I see it) should be to accept as hypotheses such propositions as those quoted above from Dr. Gundlach, to test them impartially, to reach whatever verifiable conclusions may be obtainable, and to develop inductively a comprehensive, operational, scientific theory of social change, in which should be fitted whatever of truth may be found to lie in the socialistic, as well as in other beliefs.

Even if Dr. Gundlach's position were entirely sound, the fact would remain that the capitalistic holders of power, as well as the politicians, the university faculties, the newspaper industries, and the others whom Socialists and Communists regard as henchmen of the capitalistic class, all stand in imminent and crucial peril—not so much of losing their *relative* advantage over their fellow human beings as of seeing the collapse of the entire structure of civilization upon whose continuance their present power and advantage—and even their very existence—depend. If there is such a common danger confronting all members of civilization, it would certainly seem to be a proper function of social science to seek to clarify as operationally as possible the causal factors in this crisis, and to discover what possible lines of action might have the greatest promise of averting the catastrophe and of keeping civilization in operation.

What effects a genuinely scientific study of the total situation would have upon the distribution of power, wealth, and privilege is another question. As I see it, the most wealthy and powerful people in the world today are, to very considerable extents, defeating their own most fundamental needs and potentialities. But whether or not that is true, I do not see how a genuine social scientist can merely accept the communistic dogmas about human motivation, and use them as axioms, without thereby abdicating as a scientist.

The remainder of Dr. Gundlach's paper is concerned mainly with the question of whether the values of civilization depend upon the victory of the Western democracies over Soviet Russia. It can be agreed at once that both the Soviet Union and the Western powers have at times broken their promises, have used misleading and even false propaganda, have infringed the rights of weaker peoples, have used military force and other forms of coercion when just solutions might have been reached by agreement, and have been guilty of other violations of democratic ideals. It can also be agreed (I believe) that both the Russians, the Americans, and the other peoples of the world desire deeply to preserve their cities from being bombed, to improve rather than degrade their present economic standards of living, to increase the opportunities of their youth for education, to decrease disease and eliminate preventable deaths, to cultivate the arts, to encourage joyous recreation among their people, and the like. The Soviet Communists seem to believe that these things can be accomplished only by universalizing the dictatorship of the proletariat. This system has developed out of centuries of history, during which the Russian people have had practically no direct experience with parliamentary government, civil liberties, or economic and political freedom, but have been constantly subjected to secret police, repressive censorship, and underground conspiracies.

In sharp historical and factual contrast, the United States is one of twelve nations which have demonstrated, during the past century and a third, that it is possible for democracies to work out their international relations to their mutual propserity and security without war against each other. These twelve countries include the five English-speaking nations, the three Scandinavian countries, and France, Switzerland, Netherlands, and Belgium. Each of these twelve nations has a constitution which recognizes the people, acting through their elected representatives, as the supreme authority. Their democracy is the product of centuries of experiment, experience, and evolution. Elections in these countries are competitive, with freedom to organize rival political parties and to seek to influence voters and public opinion through a free press, radio, and public discussion (distorted though these often are by financial and other pressures). Civil liberties are established constitutionally, and in spite of many serious violations are on the whole sustained.

Governments in these countries are avowedly for the public welfare. "Left-wingers" claim that the rights of private property and the interests of the wealthy have obten been maintained at the expense of human welfare; business men complain that labor unions are allowed to dictate and that the rich "get soaked" through taxation. The civil and eco-

nomic rights of Negroes are often violated—particularly in the states of the Deep South. But probably no people in all history have progressed so rapidly toward economic freedom, good housing, education, and health as have the Negroes in the United States.

All of these countries have shared in the accelerating growth of science, interchanging freely their inventions and discoveries and the applications of these to industry, agriculture, health, amusements, and other interests—particularly to military destructiveness. All have developed a swiftly growing body of trained scientific technicians and experts. Science, like government, industry, and religion, has been strongly competitive in nature but, as in these other fields, organized cooperation is increasingly taking the place of individualistic competition.

The standards of living of city workers and of farmers are higher in these twelve countries than anywhere else in the world and have been increasing more rapidly than in other nations. Death rates are lower, and expectations of life higher, than anywhere else.

Numerous factual studies of Communistic countries report that the individual citizen is regarded by the Soviet government as having no rights except as he submits to, and identifies himself with, the "party line" established by the self-appointed and self-perpetauting leaders of the proletarian dictatorship. Individual and mass murder, wholesale sabotage, plundering of occupied countries, exploitation of slave labor, complete suppression of all discussions not approved by the dictatorship —such policies are avowed or are taken for granted as instruments of the revolution. Under the democratic conception of social science, such charges as these are matters which call for impartial factual investigation—a task which has not yet been performed adequately by American social scientists. But under the Communistic control, no social scientist is permitted to hold a position, or to teach or carry on research, unless he dogmatically assumes and proclaims the immense superiority of all Communistic programs and actions over those of the non-Communistic democracies.

A major assignment for which social scientists are already vigorously laying foundations is a definitive study of the social psychology of Soviet Communism. The layman who wants to understand Russia finds himself confronted with an enormous mass of literature. Much of it is passionate propaganda. Much more is sincere but distorted arguments by fanatical Communists and anti-Communists. Other large portions of this literature are superficial, more or less sensational, poorly grounded in factual knowledge. Buried amidst this misleading

and inaccurate body of writings are numerous painstaking, dispassionate, searching studies of available facts about the history, culture, psychological attitudes, resources, ideas, and plans of the Russian people and of the Soviet government. We need a sifting of information and conclusions about the Soviet Union. We need a clear consensus of the agreements shared by the best-informed authorities, and a clear setting forth of the major issues which exist between such authorities.

Dr. Gundlach challenges the capacity of social scientists to exert constructive and disinterested influence upon social policies. My own analysis on this point would be as follows. Social action is the result of applying social power. Vast reservoirs of power exist, consisting of such factors as the number of men of military age, the gold reserve of a country, its natural resources, its factories, buildings, machines, and other productive equipment, its atomic fuels, its educated brain-power, the loyalties of its people to the government, to political parties, to labor unions, and the like. The problem is to utilize and restrain these energies in such ways as to preserve the vital structures of civilization, and also to serve to the greatest practicable degree the basic values of mankind. Since those basic values include freedom, the problem involves the attainment of the necessary control by means of consent, with the minimum necessary amounts of control by prophylactic coercion.

The uses to which national and world power are put, and the types of control employed, are determined by the directors of power, consisting of that minority which legislates, and which determines the selection of political and economic executives. The directors of energy, in turn, are held in check or released by the few great triggers of power, such as the Rockefellers, Henry Ford, the President of the United States, the chairmen of certain Congressional committees, the Secretary of State, the Supreme Court, John L. Lewis, and the like.

One of the basic tasks of social science is to understand thoroughly, dispassionately, and predictably just how these reservoirs, directors, and triggers of power operate. Hindsight keeps telling us how bunglingly these powers have been applied in the past. The survival of civilization depends upon a really adequate operational analysis of the way that social forces work, and the methods by means of which the best brains of America and of the world can team up to redirect human power into constructive—or at least not disastrously destructive—lines of action. In the future, social power may gravitate increasingly into the hands of those who assimilate and make use of a truly effective science of social relations.

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BOOK REVIEWS

ANDERSON, JOHN E. *The psychology of development and personal adjustment.* New York: Henry Holt, 1949. Pp. v+720. $3.25.

This book was designed for a general course in Human Development and Personal Adjustment in the two-year curriculum of the General College of the University of Minnesota. In contrast to a typical course in general psychology, this volume emphasizes "the genetic approach and treats adjustment in greater detail."

A little over half of the book presents concise accounts of development in such areas as physical growth, skills (sensori-motor, language, problem solving, intelligence), motivation, emotion, attitude, interest, and social behavior. The second half analyzes the adjustment process and then attacks problems of adjustment in college, in maturity and old age, in vocation, and in family life.

The most impressive thing about this excellent text lies in the way in which the author (1) develops for the student the value of an objective, constructive approach to personal problems and (2) demonstrates again and again that such an approach is possible. He does this by consistently showing that he regards the student as an adult, by selecting important issues and interesting, useful information, and by maintaining without effort professional integrity in the analysis and discussion of very difficult problems.

Here and there the particular psychological concepts with which Anderson structures his text seem somewhat irrelevant to the general aim of the book. This reader wonders whether, for example, the Lewinian concepts presented in the chapter on Motivation can contribute, without considerably more development, to the goal of "understanding at the terminal level."

The book can be highly recommended to the teacher of any introductory course which emphasizes developmental concepts and personal adjustment.

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GUTHRIE, EDWIN R., & EDWARDS, ALLEN L. *Psychology: a first course in human behavior.* New York: Harper, 1949. Pp. x+315. $3.00.

Students should enjoy this book. Its distinguishing characteristic is that it starts on the student's common-sense side of each topic and then relates the findings of psychology to that every-day experience. This strategic emphasis is implemented by Guthrie's well-known luminous style, filled with apt examples. The Flesch Ratio is close to 4, which indicates that the *potential* audience has finished eighth grade or one year of high school while the ***typical*** audience has finished high school

or one year of college. According to Flesch, college students read material of this degree of difficulty for fun.

The choice of contents, too, was apparently made with the student reader in mind. Of eighteen chapters, the first three are introductory, giving something of the development of modern psychology and introducing and discussing some aspects of science. The other two emphases of the book are on learning (including remembering), which has four chapters (68 pages), and on various aspects of personality and adjustment, which have four chapters (70 pages).

On the other hand, certain topics were apparently thought to be more appropriate for advanced courses and are treated more briefly. A 15-page chapter on "The Structural Basis of Behavior" covers material on muscles, glands, the structure of receptors, the nervous system and brain function. Sensory and perceptual phenomena in the various sense-modes are described in a 26-page chapter on "Attending and Perceiving." Descriptive statistics along with reliability, validity, and standard scores are nicely introduced and summarized on pp. 165–169. The chapter on the topics of "Heredity and Maturation" has 13 pages. The relative amount of space devoted to the other four chapters on Emotion, Drives, Intelligence, and Thinking is about the same as in other introductory texts.

In the introductory chapters, some aspects of the scientific point of view are especially well presented. The authors recognize that objectivity depends upon agreement (p. 18). They are clear about scientific determinism: "One of the assumptions that science must make is that all events are determined by antecedent conditions. This is not a belief . . . ; it is an assumption" (p. 27). And they know that science is concerned with "prediction and control" (p. 31) instead of the halfway stations of *understanding* and *explanation.*

Yet with this emphasis on the scientific point of view, there is no mention of the structure of an experiment, no mention of independent and dependent variables, nor even of "controls." The student is given no aid in constructing an experiment for himself. To the reviewer this omission is regrettable, although it may have been made in the interests of simplicity and clarity. In line with this possible plan of the authors, relatively few experiments are completely described. Experimental *results* and *conclusions* are given on almost every page, but rarely in such a way that the student reader can see how the procedure and plan lead to the conclusions. There are few tables and only 17 figures.

The treatment of *learning* presents the senior author's associative theory: In a particular situation, the organism learns the *last act performed before* the behavior sequence is interrupted by reward. To the reviewer, this description of learning differs only slightly from theories which say that any act will be learned if *followed immediately by reward.*

With only small modification the treatment in this book will be acceptable to advocates of most current theories.

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v. HOLZSCHUHER, LUDWIG. *Praktische Psychologie; die Primitivperson im Menschen.* (Practical psychology; the primitive person in man.) Seebruck: Heering-Verlag, 1949. Pp. 763.

As a result of the decline of psychology in Germany, few if any significant German contributions have come to the attention of American psychologists in recent years. For this reason, if for no other, it may prove illuminating to take a look at a rather comprehensive work which purports to deal with "practical psychology" as the German psychologist sees it. Holzschuher's *Praktische Psychologie* is a post-war contribution and presumably a representative synthesis of the German viewpoint in psychology. Its value to American readers is considerably enhanced by the fact that the author shows a familiarity with developments in American psychology, some of which he severely criticizes. As a specialist in advertising, his treatment of the effects of advertising on the public's purchasing behavior, public opinion, and attitudes is most expert. His approach is essentially "practical" but the reader familiar with German psychological literature will not be surprised at the notable lack of experimental data. Thus, the orientation is not primarily *geisteswissenschaftlich*, although large sections of the book fall into this somewhat ill-reputed category.

The book is divided into two parts, a theoretical section (about 100 pages), and a major second part, which deals with the application of the author's theoretical framework to his subject matter (about 590 pages). The latter contains chapters on the influence of unconscious factors upon various aspects of human behavior.

Holzschuher's conceptual scheme, which he calls a "strata theory," is a variety of depth psychology. Throughout he is imbued with the realization, amply substantiated by analytic psychology but not always seriously considered in academic circles, that the supremacy of the ego in directing human behavior is largely illusory. He postulates an *Ichperson* (ego) and a *Primitivperson* (id) and refers to the tensions characterizing the interaction of these systems as "bipolar." There is nothing intrinsically original about these formulations, although the author belabors the differences between his system and the theories of Freud and Jung. Nonetheless, we are presented with a consistent application of a theoretical framework to a variety of psychological phenomena, such as the behavior of social groups in response to propaganda aimed at the primitive layers of the individual personality, and people's reactions to advertising campaigns. American advertising

provides source material for the author's thesis of unconscious, primitive determinants and their effect upon behavior. It may be mentioned here that the book is handsomely illustrated, in particular with pictures from the American scene.

The fact that the ego is constantly concerned with rationalizing unconscious tendencies is of far-reaching significance in psychological experimentation. American investigations of public opinion polls and attitude measurements are criticized for their failure to take this into account. The author asserts that they usually yield merely contents of ego consciousness which are only conditionally identical with reality; for this reason it is difficult to distinguish rational from rationalized material in such procedures. In this connection, the following statement, in the reviewer's opinion, is well worth quoting: "The work of institutes of public opinion research . . . is therefore to be considered with the greatest caution, at least so long as they continue to ignore the most elementary insights of modern depth psychology" (p. 152 f.).

Students of propaganda will find many interesting first-hand observations and analyses of Nazi propaganda whose effects upon the masses are interpreted by Holzschuher in terms of its appeal to the individuals' primitive impulses. The following example is quite instructive:

> All those who rationally rejected [Nazi ideology] must admit that they were sometimes seized by a frenzy of enthusiasm which penetrated their veins and ran down their backs when they became eye and ear witnesses of well organized demonstrations. Here human bipolarity really reveals itself: The ego says "stupid trash," the primitive person jubilates (p. 245).

The treatment of personality and the emotions is studded with a wealth of anecdotal material and generalizations which are difficult to evaluate. The positive contribution of the work seems to lie chiefly in the author's penetrating analysis of Nazi ideology and propaganda, and its disastrous influence upon mass behavior. For this reason alone it deserves careful study by students of propaganda and social psychologists in general.

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THORNDIKE, EDWARD L. *Selected writings from a connectionist's psychology.* New York: Appleton-Century-Crofts, 1949. Pp. vii+370. $3.50.

In the preface to *Selected Writings from a Connectionist's Psychology,* Thorndike states that the volume was the result of a request for some three hundred pages of his most important contributions to psychology.

The book is admittedly not that, however, because he omitted from selection much of his earlier work including that on animal intelligence, individual differences, intercorrelations of abilities, the application of the scientific method to problems of learning and teaching, and also his later studies of individual differences in communities, and all of his work on tests. He favored (1) materials which are not now readily available for class reference because printed in journals or monographs and (2) books likely to go out of print. In making the selections from these sources, he states, "I have, in fact, had in mind especially the needs of teachers who wish their students to know something of connectionist psychology at first hand. . . ."

Between the Preface and Chapter I, which begins the contributions, he gives his autobiography taken from *A History of Psychology in Autobiography* (Murchison, Vol. III, 1936), supplemented by two new pages covering the period from 1934 to 1948. To the reviewer this section provides the most fascinating reading of the entire volume. In it one can discern as truly as in the scientific writings the bent of the man. As responsiveness to outer pressure or opportunity rather than to inner need is revealed as the chief source of problems of study, so also is the more outstanding personal characteristic of striving to find out what things or processes are like by experiment rather than by discussing what someone else has said they are like.

The selections are arranged in twenty-two chapters beginning with the "Law of Effect" and ending with "Darwin's Contribution to Psychology." In between are covered subjects almost as extensive as psychology itself. Several chapters deal with highly technical problems involving variables bearing on the establishment of *connections*, while others treat problems of a more general nature such as personality, labor, mental fatigue, language, and values. The forty-odd selections were taken from eight books, seventeen scientific periodicals, one popular magazine, and three "memorial" volumes. The overall range in publication dates was from 1905 to 1947, with two-thirds of the articles being originally published after 1931. Lengths of reprints vary from three to eighteen pages with the average around nine.

While it will be possible for a student to learn from *Selected Writings* much about Thorndike's connectionism after 1930 and less about his connectionism before 1930, the more thorough investigator will not be content with the brevity and sketchiness of many of the reprints. The extensive topical coverage and the lack of coherence of the chapters indicate, however, that the author did not intend the book for that purpose. Its greatest usefulness will be in the hands of students not specializing in the research aspects of learning and motivation but rather in their applications.

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WEVER, ERNEST GLEN. *Theory of hearing.* New York: John Wiley, 1949. Pp. xiii+484. $6.00.

Professor Wever's book is addressed to the difficult and challenging problem of evaluating the so-called "place," "frequency," and "place-frequency" theories of hearing from a historical point of view and in the light of modern anatomical, neurological, and psychological evidence. This is a major contribution from one of the principal investigators in the field of hearing, culminating over two decades of continuous study and research on the subject. It is written in a straightforward and lucid style.

Part One of *Theory of Hearing* sets forth the classical and present day theories of hearing with critical insight into their shortcomings. Here more than a score of theories responsible for stimulating and guiding thought and research in audition are arranged and discussed with distinguished scholarship.

On this firm historical basis, Part Two takes up the modern developments of classical theories. The impact of new knowledge of the detailed structure and dynamics of the cochlea and the electrophysiology of the auditory nerve upon the classical Helmholtzian theory of sound analysis is demonstrated. In this same section the "volley" principle of nerve action is presented as a phenomenon which must, it is argued, supplement and to some extent supplant the theory of localization on the basilar membrane as a mechanism whereby the auditory system achieves an analysis of frequency; the author feels that the latter theory has gained an undeserved popularity. Wever takes the position that the principles of "place" and "frequency" should not be considered necessarily as conflicting and that they can be harmoniously combined to achieve a fairly comprehensive and workable theory of hearing. Although considerable attention is given to attributes of audition other than pitch discrimination, this theme dominates the book.

In Part Three, which constitutes over half the book, the author presents his "resonance-volley" or "place-volley" theory of audition. "Resonance" in the physical sense of sympathetic vibration of any cochlear structure is discounted, but variation of place of stimulation on the basilar membrane as a function of the frequency of a sound wave is held to account for discrimination among tones of high frequency (above about 4000 cps); the analysis of low frequency sounds is made possible, it is proposed, at a cortical level due to a somewhat faithful reproduction of the wave form of sound stimulation by a patterning of auditory nerve impulses according to the principles of nerve "volley" activity. This proposed dual mechanism is based on well-documented evidence that the conditions necessary for frequency localization within the cochlea are met and, likewise, it is established that to some degree the auditory nerve discharges at rates that correspond to the frequency of sinusoidal sound stimulation up to near 4000 or 5000 cps.

The experimental evidence on acoustic neurology, sensitivity, loud-

ness and fatigue, pitch and pitch discrimination, auditory abnormalties, tonal interaction, temporal phenomena of auditory perception, and binaural phenomena are marshalled before the reader in an orderly fashion and cast against the "place-volley" theory for explanation. Original data hitherto unpublished will be found here. Finally, the book ends with a section of definitions and symbols and a select bibliography of about five hundred titles.

It is to be expected that there are those who will disagree with the interpretation and treatment given some of the auditory data and feel that insufficient facts are available concerning cochlear function to warrant the emphasis placed upon the proposed role played by volley action within the auditory nerve. However, all students and investigators of hearing will find *Theory of Hearing* an important book as a source of information and stimulation that will have a significant influence upon future auditory research.

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RUBIN, EDGAR. *Experimenta psychologica.* (Psychological experiments.) Copenhagen: Ejnar Munksgaard, 1949. Pp. 356. 3100 kroner.

"Friends and students have expressed their interest in the publication in collected form of the scientific papers which I have published in various periodicals. This has led to the present book." Thus does the author state the purpose of this reprint of fifteen of his articles published between 1911 and 1938. Eleven of them are in German, two in English, and two in French. Rubin speaks of his own as "adspective" psychology which is the opposite of structuralism and yet not quite like the orthodox Gestalt psychology to whose early development he has contributed so significantly with the figure-ground concept. Adspective psychology is concerned with aspects of wholes which are not to be artificially analyzed and subsequently synthesized. One does not know how many aspects there are or can be. Classification of these aspects is always tentative, as new material will often make regrouping desirable. Such a point of view runs counter to the modern tendency toward systematization in psychology.

Almost one half of the volume, 170 pages, is devoted to the single problem of how a straight edge is experienced by vision and touch. Other problems treated by the author are the latency between stimulation of the retina and awareness of the stimulus, the visual perception of actual movements, an auditory complication experiment, and time, vibratory, temperature, and taste sensations. A few short articles are concerned with theoretical questions, and one deals with an aspect of the philosophy of Hoefding, his teacher.

The total impression gained from this volume is that of the life work of a psychologist who was a lucid writer, a careful experimenter, and an

independent thinker who early in his career went his own way. It is probably safe to predict that Rubin's lasting reputation will not rest on the contributions which are here reprinted, but on his *Visuell wahrgenommene Figuren.*

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CROW, LESTER D., & CROW, ALICE. *Educational psychology.* New York: American Book Co., 1948. Pp. xiv+599. $4.00.

The unusually detailed and systematically organized table of contents of this text well displays the range and arrangement of topics touched upon. The 29 chapters are grouped into seven parts. After two introductory chapters, Part II considers Growth and Development, with separate brief chapters given to the general nature of growth, and to physical, mental, emotional, and social development. Part II on "The Learner's Potentialities" deals successively in chapters 8–11 with intelligence, aptitudes, personality traits, and individual differences. Six chapters then treat learning: general principles, attitudes, study habits, motor skills, thinking, and transfer. Part V in three chapters considers the nature, scope, and techniques of evaluation, and statistics. Six chapters on the "Psychology of Learning Areas" are devoted to the following topics: "developing skill in spoken language" and in reading, spelling, handwriting, foreign language; the "Psychology of Arithmetic" and of algebra and geometry; the natural sciences in the elementary school and in the high school, and similarly the social sciences at both levels; the development of appreciation in art, music, and literature; educating for health and safety; and "Vocational Exploration and Preparation." A concluding part on adjustment considers exceptional individuals, behavior drives, and mental hygiene. Obviously, the authors have attempted to cover an exceedingly wide range of topics. The dominant emphasis, as stated in the preface, is upon the guidance of development toward good adjustment.

With so many and various topics to be dealt with, inadequacy in the treatment of at least some would seem hardly avoidable. The course of physical growth from birth to maturity is presented in 9 pages, with two charts. The psychology of reading, including remedial teaching, is considered in 5½ pages, plus two plates showing an ophthalmograph and records obtained therewith. Spelling and handwriting receive each three pages, social sciences in elementary and secondary school a total of 4 pages, "vocational exploration and preparation" 7 pages. As is perhaps inevitable when so much is attempted, large use is made of secondary sources and some rather old material. Treatment may become superficial, even platitudinous (thus we are told that "Sufficient restful sleep is healthful" in the two pages on "physical well-being" in the chapter on mental hygiene). Over-caution in brief statements may omit and seem to reject present best-informed attitudes on a subject; thus a one-page discussion of "the sex urge" could almost have

been given to a Sunday school class in 1890—though later comments on "sex adjustment" suggest a more tolerant understanding.

The volume is, however, a well organized display of the variety of topics in the total broad field of educational psychology, and is written with an evident sincerity of interest in children and their welfare. Each one of the 27 chapters ends with "questions and topics for discussion" and a list of "selected references" as aids to instructor and student. The author and subject indices seem more carefully made than in many similar volumes.

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ROSENZWEIG, SAUL (with KATE L. KOGAN). *Psychodiagnosis.* New York: Grune and Stratton, 1949. Pp. x+380. $5.00.

In this book the authors present an introduction to methods of psychological diagnosis of individual personalities. Their aim has been to provide a variety of concrete illustrations which exemplify the conceptual and clinical operations actually performed by the modern, or dynamically oriented clinical psychologist. The authors de-emphasized abstract formulations of theory and method "partly because it has seemed premature to state baldly principles that are as yet largely implicit and tentative in the work of the psychodiagnostician" (p. viii). Abstract and theoretical formulations have by no means been disregarded, however, in the attempt to avoid jargon.

The three major sections of this book introduce the reader to diagnostic methods by demonstrating, in steps of increasing complexity and scope, the manner in which the working clinician uses and integrates test results and other information. First, under the heading of "The Range of Available Instruments," typical and popular tests for the assessment of intelligence, deterioration, vocational aptitudes and interests, and personality are described and illustrated by protocols and discussion. The next section, "The Process of Psychodiagnostic Integration," presents test results and protocols for four different cases. The discussion of each case examines the contribution of particular tests to the diagnosis, as well as the proper combination and correlation of test results to provide an integrated picture of each personality. This section also includes a nice discussion of the art and practice of writing psychodiagnostic reports. In the section, "A Variety of Applications," the authors present additional cases to give the process of diagnostic integration more extended treatment. This section emphasizes in greater detail the contribution of social and historical data to the understanding of the total individual.

The general introduction, "Clinical Psychology as a Psychodiagnostic Art," presents the basic frames of reference which influence the authors throughout the book. Here the authors distinguish between the more old-fashioned psychometric approach to diagnosis, which they seem to consider as depending largely upon rigid and stereotyped infer-

ence from tables of norms, and more modern approaches. The latter emphasize highly individualized interpretations which rely heavily upon inferences from qualitative aspects of objective test performance and projective test results, all in accordance with dynamic theories of personality. This does not mean that the authors deny the importance of conventional norms but rather that they emphasize, in a quite specific fashion, the relative and provisional contribution such normative data make to the understanding of an individual person.

In the closing chapter on "Psychodiagnosis as a Science" the authors view with favor the beginning rapprochement between experimental and clinical psychology. Here they make a case for the individual, in all his complexity, rather than the "generalized average man" (*sic*) as the more profitable unit for purposes of scientific scrutiny. The discussion in this chapter seems almost to identify scientific enterprises devoted to the discovery of general laws of behavior with the production of propositions about "the generalized average man" or about isolated segments of behavior which will be irrelevant to the task of understanding the total human individual. Though such an outcome has been frequent, possibly even modal, this reviewer is inclined to question any implication that it is either necessary or unavoidable. After all, psychoanalytic theory, which is widely conceded to be relevant to the clinical enterprise of understanding the individual, is a general theory of human behavior and development. And recent advances in learning theory, based largely upon isolated segments of the behavior of the rat, appear not only to confirm some aspects of psychoanalytic theory, but even promise to furnish clues as to how it may be revised and extended.

In general, the discussions as well as the test and case materials are well formulated and lucidly expounded. They should prove effective as an introduction to the sophisticated clinical approach to psychological diagnosis. The authors deserve commendation for their attack on this difficult educational task. Though a text of this length cannot cover, or even mention, all of the popular testing techniques, the selection in this book seems sufficiently broad and representative for introductory purposes.

For all its virtues, the book has several serious limitations in its role as an introductory text. For one thing, the interpretations and inferences included in the discussions range from the probably valid or widely accepted and canonical, through the hypothetical, to those which the authors would, no doubt, be willing to class as frankly speculative. Even so, alternative interpretations and qualifying statements appear only infrequently. The beginner, without extensive background in clinical psychology, will find it difficult to evaluate the validity of these interpretations and inferences. Little in the book would even suggest that such a problem exists. Furthermore, the bibliography includes only very scanty reference to the critical research and clinical literature. Though space limitations imposed on such a book preclude detailed

examination of the validity of the interpretations presented, the general problem deserves more emphasis than it received. For pedagogical reasons some attempt should have been made to differentiate between standard, widely accepted interpretations and those which are hypothetical. This could have been achieved, without swamping or discouraging the beginner, by means of brief, organized discussions illustrated by some research data and argument, all presented with the same lucidity the authors achieved in delineating their approach to psychodiagnosis.

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STECKLE, LYNDE C. *Problems of human adjustment.* New York: Harper, 1949. Pp. x+351. $3.00.

This text is offered by the publishers for courses in mental hygiene, personal adjustment, and personality development. The author states that the volume represents the concepts he has found to work when the attempt is made to introduce man to himself.

It suffers grievously from the lack of a systematic conceptual framework, and the net effect is chaotic. It is evident, for example, that the author has not sought for essence in the field of motives. Contradictions abound. While he urges acceptance of man's animal nature, which he identifies with emotion, he seems unable to see emotion as anything but bad. His casualness with this concept as with others creates trouble here as in many other places. Emotion and learning are flatly called antithetical. It is said that under heightened emotion one learns with difficulty. Yet fear and anger as habits are maintained because they assist the person in the attainment of his goals.

One fears that the young undergraduate would be led to think that the best he could do with his own emotions would be to sit on them. Emotions like delight and affection are mentioned, but briefly, and they are given slight roles in conduct. It will be hard to convince the undergraduate, too, that "the biological act of intercourse is a not too important part of what the term love has come to imply."

The author maps "The Road to Neurosis" in a chapter, and it is of interest that nowhere does the road traverse the domain of Eros. Eros does not even go on the journey as guide or companion, nor is he granted the honor of being a symbol of that security and self-esteem which all the travelers, although strayed from the proper road, are held to be seeking.

Although his world sounds in many ways like a comfortable one where the main problems of life are really pseudo-problems which "do not exist for the well-adjusted individual," the author develops a degree of hortatory zeal in urging a rational course upon man, and a vehemence in castigating him for his stubborn irrationality, that would please the soul of many a one of the orthodox preachers who are among the groups

most vigorously thrashed. Rules, guides, and precepts for realistic and efficient living are liberally set forth, although the author repeatedly professes complete disbelief in the efficacy of telling man what he should do.

Students, undergraduate or other, will thrive better on more profound, more deeply searching treatises than this one.

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MCKINNEY, FRED. *The psychology of personal adjustment.* (2nd Ed.) New York: John Wiley, 1949. Pp. xi+752. $6.00.

Freshmen in college are in position to profit from a knowledge of psychology before taking general psychology, usually not given until the sophomore year. Aware of this need, McKinney in 1941 wrote the first edition of the present volume which, in his words, was "written to meet the need for a basic psychological text which frankly attacks the problems of the student that are most vital to his personal adjustment and which offers him factual material on these problems." The content of the second volume in general is identical with the first, with relatively few changes. The addition of a chapter on development of personality and the addition of more recent references are the most noticeable changes.

There are a number of striking features in the book. One is a large amount of illustrative case material; another is a comprehensive list of references; still another is the inclusion of many lists which offer in summary fashion advice or maxims for living. The book includes lists which give a college student's reasons for going to college; a check list for effective study habits; one giving motives for the individual; and another a list of issues for a personal philosophy of habits which make us likeable; advantages and disadvantages of fraternity life; and offensive and inoffensive ways of handling different situations. Advice is offered on ways of taking exams, budgeting time and many diet and health habits, developing a personal philosophy, developing leadership, and controlling masturbation. Counseling is favored because discussion provides an outlet, de-sensitizes the individual, changes his attitudes, discovers repressed material, helps an individual organize and develop self-acceptance and motivation. All of these are described in brief as assets and potentials of counseling. The dynamics of the process by which such virtues are obtained are barely mentioned. In the chapter devoted to the need for a philosophy of life, Mencken, Keith, Hu Shih, Lilienthal, and Mumford are quoted, and fairly effective use is made of these quotations for suggesting a need for attaining a philosophy of life.

The emphasis on growth and maturity is not as dynamically presented in the material considered as the students obtain from McKinney's courses. He belongs to that group of good teachers who influence by radiation as well as by presentation. There is material in this book

which can be used to advantage by students who are on the search for knowledge about life and even for gaining some insights which will make it possible for them to get a look at life in a manner that is forward-looking, healthy-minded, and integrated rather than merely adjusted—though adjustment, not integration and integrity, is the emphasis of the book. All in all, though, it can serve very well the purpose for which it is intended—a helpful volume for the person who is starting on the road towards leading a college life which promises well for his growth.

H. MELTZER.

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ALMY, MILLIE CORINNE. *Children's experiences prior to first grade and success in beginning reading.* New York: Bureau of Publications, Teach. Coll., Columbia Univ., 1949. Pp. viii+124. $2.35.

GRAY, WILLIAM S. *On their own in reading.* New York: Scott, Foresman, 1948. Pp. xvi+268. $2.00.

WITTY, PAUL. *Reading in modern education.* Boston: Heath, 1949. Pp. xvi+319. $3.50.

These three books cover phases of reading, a topic which has been widely discussed in professional literature for the last few decades. One of the books, *Children's Experiences Prior to First Grade and Success in Beginning Reading*, is an experimental study. The other two, *Reading in Modern Education* and *On Their Own in Reading*, are discursive presentations aimed at helping the teacher to do a better job in the teaching of reading. They could both be used as textbooks in university course work.

Reading in Modern Education covers the whole field of reading from the development of readiness in the preschool years to at least a mention of reading problems of college students. The last chapter is a summary statement concerning the author's own philosophy of the importance of reading and its status in the future.

Children's Experiences Proir to First Grade and Success in Beginning Reading is an attempt, on the part of the author, to determine experimentally whether the factors which have been considered to be important in the development of readiness actually do produce the effects desired. As far as experimental design is concerned it is somewhat questionable whether the author has been able to prove her point, but it is good to review these factors and call them again to the attention of those who read the book. The difficulty is that, cast in this style, it probably will have very little reading public. Yet written for the potentially more effective public—parents—it might receive wide attention. The author concludes that "This study has substantiated the fact that there is a positive relationship between the child's responses to reading opportunities prior to the first grade and his success in learning to read (p. 114). . . . The correlation would never approach the point where

beginning reading success could be predicted on the basis of preschool reading experiences. There are too many other contributing factors" (p. 117). The author goes on to state that even if exact prediction cannot be made, it is important to know that this relationship does exist.

On Their Own in Reading is an illustration of a book written to a purpose and satisfying that purpose very fully; that is, to aid teachers in the development of lesson plans and specific classroom exercises which will bring about more effective classroom instruction in word attack and thus develop more independence in reading. This topic is mentioned and given some attention by Witty in his discussion of reading in general, but Gray has not only told the teacher what to do, he has shown her how to do it. This is something which is so often not effectively done. In education today we have a tendency to talk at the students instead of talking with them and demonstrating ways in which topics covered by our lectures may be carried out, thus taking the first step toward helping the teacher to put them into practice.

There is little doubt that the teacher who takes the trouble to read *On Their Own in Reading* will at least try some of the techniques so ably presented. A sound psychological approach to the teaching of reading is well illustrated. For instance, psychologists recognize the soundness of the philosophy that the background of the individual is a potent factor in learning. Dr. Gray does not let his readers forget this. He gives illustrations where this principle might otherwise be overlooked. Thus, there is the reminder that the teacher must start teaching with words in the spoken vocabulary of the child for, he says, at no time does the child get material solely from printed symbols. He must at all times bring meaning to them. In much reading instruction and formal course work, we have a tendency to forget these basic principles and make presentations appear to stand alone, forgetting that the background of the individual affects markedly what he obtains from the material presented.

While the other two books cover only restricted phases of the field, Witty leaves almost no topic untouched in his presentation. It takes both kinds of books to present a working approach to the subject. Witty's somewhat cursory presentation of some topics will serve as at least an introduction. He has presented bibliographical references which will help the reader to go to more detailed presentations such as those by Almy, Gray, and others to get the details which would be necessary if the topics discussed by Witty were to be well understood. At times his presentation is little more than a full outline of his subject. This is a good illustration of where his reader must bring a good deal to the topic presented if this book is to be fully understood. It is well to mention that Witty ably makes a balanced, concise presentation of such topics as the "non-verbal" child, "dominance," the use of mechanical gadgets in the teaching of reading, and other such subjects which have for too long

caught the fancy of some persons in the field. They have built theories of the psychology of reading around these restricted considerations and thus base their instruction on somewhat narrow concepts rather than on a total over-all sound theory of what is known of the psychology of reading. Witty's very brief section on reading as it is related to the total psychological development of the child is also well done and should be of special interest to both parents and teachers.

FRANCES ORALIND TRIGGS.

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